Vol. 49, No. 2 March, 1942

# Psychological Review

EDITED BY
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PUBLISHED BI-MONTHLY
BY THE
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
PRINCE AND LEMON STS., LANCASTER, PA.
AND NORTHWESTERN UNIVERSITY, EVANSTON, ILLINOIS

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

PUBLICATIONS OF

# THE AMERICAN PSYCHOLOGICAL ASSOCIATION

WILLARD L. VALENTINE, *Business Manager*

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# THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

NORTHWESTERN UNIVERSITY, EVANSTON, ILLINOIS

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VOL. 49, NO. 2 MARCH, 1942

# THE PSYCHOLOGICAL REVIEW

## KURT KOFFKA

1886–1941

In 1910 Kurt Koffka was assistant at the Psychological Institute in Frankfurt am Main. In the fall of that year the present writer became second assistant at this Institute. The winter term had hardly begun when Max Wertheimer appeared with a primitive stroboscope in his suitcase and with many ideas in his head. At the time none of us knew much about the two others; but Wertheimer stayed, and working together we became the first three Gestalt psychologists.

Those were years of cheerful revolt in German psychology. We all had great respect for the exact methods by which certain sensory data and facts of memory were being investigated, but we also felt quite strongly that work of so little scope could never give us an adequate psychology of real human beings. Some believed that the founding fathers of experimental psychology had done grave injustice to every higher form of mental life. Others suspected that at the very bottom of the new science there were some premises which tended to make its work sterile. The healthy effect of all this dissatisfaction was that, as everybody tried to prove his point, the younger generation turned to observational material to which the older generation had paid scant attention. The Würzburg School contrasted '*Aufgaben*,' 'determining tendencies' and 'imageless thought' with mere sensory content and with passive automatisms. In Göttingen Katz, Rubin and Jaensch began to investigate striking perceptual phenomena, happily unconcerned with the question whether they were dealing with true sensations. Moreover, Ehrenfels' *Gestaltqualitäten*, which in the Nineties had been roughly

treated, began to arouse considerable interest in the new atmosphere of freedom and expansion.

Koffka, of course, was among the reformers. He had taken sides before he went to Frankfurt. In fact, his first book—on *Vorstellungen*—was very much a product of Würzburg although he published it in 1912; but even before this book had gone to press he had begun to feel dissatisfied with the Würzburg reform. Apparently the Würzberg School was quite willing to accept after minor corrections the traditional doctrine of sensory function and of automatisms in memory. Moreover, it seemed to Koffka that little progress was being made as to the positive principles which the higher functions, thought and intentions, actually obey.

In Frankfurt Wertheimer worked on stroboscopic movement, which many would regard as a minor problem in perception. However, both his experimental analysis and his theoretical procedure were entirely unusual and made the very deepest impression on Koffka. He recognized at once that the implications of Wertheimer's work extended far beyond its special subject and also beyond any problems of perception. This opinion he found amply confirmed by Wertheimer himself whose ideas were rapidly developing in investigations of perception, of thought processes, and of the symptoms of brain lesions.

At this point young Gestalt psychology had obviously two tasks. In the first place its empirical basis had to be amplified. In the second place the general principles involved in Wertheimer's work on movement had to be established *per se*, independently of this particular application. Koffka's first contributions followed the former line. After Wertheimer's pioneer research visual movement seemed to him a particularly promising field. Thus he and his collaborators began a long series of experimental investigations which revealed not only new aspects of movement as such but also important relations between movement and other perceptual facts. Korte's laws were established, the characteristics of $\gamma$-movement were discovered, and fusion of successively given fields was proved to depend on their structure. Almost at

once, however, it became apparent that not all psychologists were willing to follow. For the most part their arguments rested on traditional presuppositions which Wertheimer and Koffka no longer accepted. In this situation experimental research alone did not suffice as an answer to the critics. It had to be made quite clear that the new point of view meant a radical change as to some basic assumptions of psychology. Koffka was therefore forced to explain the general principles which Wertheimer and he followed in their work. His formulations are contained in a discussion with Benussi (1915). Briefly, their import is this:

1. An atomistic interpretation of human experience in terms of ultimate and simple elements is incompatible with the very nature of this experience. The primary data of perception, for instance, are typically specific structures or Gestalten.

2. Functional concepts must follow descriptive facts. In perception, therefore, it is misleading to correlate stimuli with sensations. Rather patterns of stimuli are to be correlated with those specific structural entities which are the essential content of perceptual fields.

3. With these premises the problem of psychophysical relationships appears in a new light. In place of the traditional view that brain processes are aggregates of elementary local excitations, it is postulated that brain function tends to assume the form of specific molar events which correspond to the structures found in experience.

In this form the scheme still showed clearly that its origin lay in studies of perception. But Koffka himself proved its general relevance when a few years later he wrote *Die Grundlagen der Psychischen Entwicklung*, a book which American readers know as *The Growth of the Mind*. Here the main principles appear in a much more definite shape, and therefore they can now be concretely applied to the most varied psychological topics from eye movements to thought processes.

At about the same time Koffka undertook the difficult task of introducing Gestalt psychology to American psychologists. His article in the *Psychological Bulletin* (1922)

attempted to explain the difference between Gestalt principles and other views in a discussion of perception. Now, it is true that *initially* this difference could best be clarified through reference to instances from perception. But masterful as Koffka's presentation was it probably led to the erroneous belief that Gestalt psychologists unduly overrate perception and have little to offer in other parts of psychology. A second article by which this impression might have been destroyed was planned but never actually written.

I will not deny that so far as his own work was concerned Koffka preferred the investigation of perceptual problems to any other experimental topics. This preference had methodological reasons. It seemed to him that for the time being clear decisions could be more easily attained in this field than in other branches of our science. But then we must remember that to a Gestalt psychologist an investigation of perception has relevance to topics beyond perception in the narrower sense. For to him it is becoming more and more apparent that in various forms and modifications the functional principles involved in perception apply to wide ranges of psychological facts. In this sense we have to understand the penetrating work of Koffka and his collaborators at Smith which for the most part referred to color vision in its relation to perceptual organization.

In the meantime Koffka had become an American. As an American he was sensitive to the fact that many psychologists in this country did not like the ways of Gestalt psychology. But he hoped that with fuller knowledge of its factual material and its arguments the hostile attitude would disappear. Undoubtedly this was the motivating power which enabled him to write his great *Principles of Gestalt Psychology*. In this volume perception is still in the foreground. But in four chapters on learning and on memory it is made convincingly clear that Gestalt psychology is gradually conquering these other fields. In fact, these chapters, which seem to be written with particular ease and joy, may be counted among Koffka's strongest contributions to psychology.

*The Principles of Gestalt Psychology* is a bold attempt at systematization. Throughout its fifteen chapters the author is struggling for a unitary view of his data in the conviction that ultimately psychology will be found to stem from but a few powerful roots. On reading the first and the last chapter, however, one will soon recognize that with this conviction there goes another belief. It has been said that Gestalt psychologists have motives and hopes which are not grown on scientific ground. Koffka might have answered: "Why, yes. How could it possibly be otherwise? As a human being I know of many things; and that special tool of mankind, the science of our time, applies only to some. When used in handling the most important topics such as 'relevance,' 'sense,' 'good' and 'wrong,' the tool has lately been found to destroy rather than to be helpful. This being so I have more confidence in my own awareness of such matters than in the working of the blundering tool. Obviously, for these matters we need a better tool. And nothing gives me more confidence in Gestalt principles than the fact that here at last relevance, sense, and intrinsic value appear with natural ease among the characteristics of our subject matter."

In a fine sense Kurt Koffka was a proud person. I have never seen him afraid of anything. To a few he was the most loyal friend. Everybody found him generous and just.

WOLFGANG KÖHLER

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# EXPECTANCY VS. EFFECT—SUBSTITUTION AS A GENERAL PRINCIPLE OF REINFORCEMENT

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## I. The Failure of Expectancy to Explain Reinforcement by an Unexpected but Desired Outcome

Hilgard and Marquis, in their excellent discussion (3) of reinforcement, contend that neither the principle of effect nor the principle of substitution is completely valid. They suggest that if we are to secure a single law which will explain all cases of reinforcement we must turn to the principle of expectancy. According to this principle, "reinforcement must be such as to confirm an expectancy" (3, p. 87). It is assumed that an animal "'expects' the goal to appear if it follows the 'behavior route.' . . ." "If the goal materializes the expectation is confirmed, and when the situation is repeated similar behavior sequences will again be evoked. If the expected consequences do not materialize, failure will lead to varied behavior on the next trial" (3, p. 88).

Common sense leads one to the 'expectancy' that this principle will have trouble whenever the expectancy is at odds with the 'hopes,' 'wishes' or well-being of the organism. If by some response the organism stumbles unexpectedly on a valuable outcome, will the failure of the response to confirm an expectancy prevent it from recurring? Conversely, if a response is followed by an expected evil will the confirmation of this expectation reinforce the response?

Conditions such as these may occur when students are required to guess at answers to questions. A student who knowingly makes a wild guess at a question must obviously expect to be wrong, but presumably prefers to be right, or would be pleased if he were right.

To see if this common sense expectancy is confirmed, we may consult some data obtained from an experiment directed to another purpose (5). In this experiment 78 high school boys were required to take the same multiple choice examination on four successive days. On each of the first three days the subjects indicated for each of the 84 questions both their choice and their degree of certainty. On the first two days no information regarding the correctness of the choices was given. On the third day, however, in the case of most choices, the student was informed immediately after making each choice whether he was right or wrong. For certain control choices no information was given. The test was then repeated on a fourth day to see how the information affected the persistence of the choices.

Of the total of some 6000 choices there were 781 which appeared for the first time on the third day. Of these there were 232 which were rated 'Guess' on the third day. It would seem that all 781 choices would be accompanied by the expectancy of receiving 'Wrong.' This would seem especially so in the case of the 232 choices of which the subject was so unsure as to pass up on the first two days and to label 'Guess' on the third day.

The results in the table fail to support the principle of expectancy. Securing an outcome which was expected but

TABLE

The tendency to repeat choices after the undesired expectation (UE) of being told 'Wrong' is confirmed and after it is reversed.

| | Choices appearing for the first time on day three | | | |
|---|---|---|---|---|
| | 'Guess' Choices | | All Choices | |
| | No. of Choices | Per cent Repeated on Day 4 | No. of Choices | Per cent Repeated on Day 4 |
| UE Confirmed (Told 'Wrong') | 62 | (*c*) 34 | 215 | (*c*) 26 |
| UE Neutral (Told Nothing) | 114 | (*n*) 34 | 350 | (*n*) 40 |
| UE Reversed (Told 'Right') | 56 | (*r*) 66 | 216 | (*r*) 70 |
| Net Reinforcement from Confirmation | (*c*−*n*) | 0 | | −14 |
| Net Reinforcement from Reversal | (*r*−*n*) | 32 | | 30 |

presumably not desired does not reinforce the tendency. The responses securing such outcomes are repeated no more frequently (if anything, less frequently) than responses followed by no definite outcomes. On the other hand, an outcome which is contrary to one's expectation but in line with one's wishes markedly strengthens the connection. Responses followed by such outcomes are repeated much more frequently than responses followed by no definite outcomes.

We may sum up our criticism, somewhat tritely, as follows: It is not sufficient for an outcome to confirm an expectancy. The outcome must also be valuable or acceptable. It is not necessary for an outcome to confirm an expectancy. An unexpected but valuable or satisfying outcome will produce reinforcement.

It is possible, however, that our whole criticism is based on a too literal interpretation of expectancy. Perhaps when the authors speak of an outcome confirming an expectancy, they mean the confirming of an acceptable expectancy. Perhaps also under a more liberal interpretation expectancy would include not only outcomes which the organism considers probable, but any contemplated outcome which is acceptable or satisfying, any hope no matter how forlorn. Under such a broad interpretation the winning of a 5000 to 1 sweepstake, for instance, would be the confirmation of an expectancy, since in this way the individual would secure a contemplated and acceptable, or satisfying outcome.

It is doubtful, however, that this liberalized interpretation of expectancy was intended by the authors. It is doubtful because this interpretation, by stressing the value, satisfyingness or acceptability of the outcome, reintroduces the principle of effect. By such an interpretation, in fact, expectancy becomes merely a supplement to effect and ceases to be the rival principle clearly intended by the authors.

If expectancy were intended merely as a restriction on effect, it seems necessary, moreover, to point out that it would be a restriction of questionable validity. The restriction states that a reinforcing outcome in addition to being valuable or satisfying, which effect has all along maintained,

must also be contemplated. I believe that even an uncontemplated valuable outcome will reinforce a tendency. If a young man in a purely casual way, had taken a new route home and had unexpectedly come upon an unusually attractive girl, it is quite conceivable that he may choose that route again. I believe this would be true, even if upon first taking the route he had never contemplated the chance of meeting the girl.

## II. A Two-Phase Integration of Effect and Substitution as a General Principle of Reinforcement

If this criticism of expectancy is valid we must turn elsewhere for a universal principle of reinforcement. To the writer the most promising general principle is a hybrid, or two-phase, principle made up of effect supplemented by substitution. Such an integration is suggested by the fact that the most pronounced failure of each of these principles coincides with the most indubitable success of the other.[1]

There may be considerable disagreement, however, as to the most pronounced defect of the principle of effect. The defect stressed by Hilgard and Marquis, I cannot take very seriously. These authors are concerned about the failure of effect to describe instrumental avoidance learning (learning to lift the foot to the sound of a whistle and thereby avoid a shock). They suggest that for this sort of learning the principle of effect "provides no simple explanation, since the shock cannot be considered a reward, and the absence of shock can be rewarding only if some sort of expectation of shock has been established in the subject" (87).

It would appear, however, that the explanation offered by effect is not unduly complex or indirect. To explain avoidance learning, effect has merely to resort to a notion always implicit if not explicit, in any trial-and-error theory of learning. This is the notion of alternative or competing con-

[1] I tried to derive or extract this two-phase principle from Hull's second postulate (Mind, mechanism and adaptive behavior, Psychol. Rev., 1937, 44, 1–32), but, failing to make much headway, decided to attempt an independent development. I would not be greatly surprised, however, if everything in the two-phase principle can be derived from the second postulate.

nections. Trial-and-error learning can take place only if there is some other connection waiting to take over when the 'erroneous' connection has been eliminated. According to the trial-and-error theory, an animal in a new situation (*CS*) responds by $R_1$. If this fails to bring valuable results the tendency $CS$–$R_1$ is eliminated and $CS$–$R_2$ takes its place. Where did $CS$–$R_2$ come from? It must always have been available as a potential or alternative connection to $CS$–$R_1$.

If this essential notion of alternative connections is brought into the open, it would appear to account for avoidance learning. If all connections competing with $CS$–$R_n$ have been stamped out, it would seem that $CS$–$R_n$ would persist. In this case, moreover, it would seem that if any other connection except that leading to foot lift had operated it would have been stamped out since any other response, or any failure to make this response, would have been followed by a shock.

A more serious difficulty with the principle of effect would seem to be its failure to predict the early sequences of learning. Let us suppose that in the presence of some such *CS* as 'being in the box' the animal responds by pressing the lever and that this response is followed by the appearance of food (*US*) at a *distant* food chute. To this *US* he responds (*UR*) by a vigorous approach to the food chute. After a *great many trials* the animal will, of course, approach the lever almost immediately upon being put in the box. But in the *early trials* there is a strong probability that upon being put in the box (*CS*) he will first approach the food chute (*UR*), and only after some unsuccessful behavior around the food chute will he trip the lever. It is this tendency to approach the food chute during the early trials that the principle of effect fails to predict.

This failure is inevitable from the nature of the principle of effect. The principle of effect merely states the conditions under which a tendency will be strengthened or weakened. Once a tendency has been weakened to the point of extinction there is nothing in the principle to predict the connection which will succeed it. It is true that if the number of probable competing responses is very limited, as is most likely to be the

case in avoidance learning, we can predict that the next connection will be one of these. If the number of competing responses is large, however, and if these, as far as we can tell, are equally probable, effect provides no means of predicting the successor to the eliminated connection.

Situations in which there are a large number of alternative responses of almost equal strength appear very often in the school room. Let us consider the following dialogue:

Teacher: "Who wrote . . . ?" (*CS*)
Pupil: 'Shakespeare.' (*R*)
Teacher: "No! Bryant wrote it." (*US*)
Pupil: (aside) 'Bryant?' or, 'Oh, yes, Bryant!' (*UR*)

The principle of effect predicts that the tendency to answer this "Who wrote" question by saying 'Shakespeare' will be weakened by the application of *US*. It predicts that the next time that particular question is asked the pupil is likely to give the name of *some other author*. But which other author? On this point the principle of effect is silent. From observations, however, we know that there is a fair chance that the pupil will say 'Bryant' next time.

This prediction which effect fails to make, is the outstandingly successful prediction of the principle of substitution. And, on the other hand, the successful prediction of effect coincides with the most dubious aspect of substitution. The principle of effect neatly describes the final adaptive stages of learning, but fails to describe the initial stages. The principle of substitution, on the other hand, neatly describes the initial stages, but fails to describe the final adaptation in those cases, such as that described, in which *UR* (going to the *food chute*) is not an adaptive response to *CS*.

Not everyone, of course, admits that substitution fails to account for final adaptation in cases such as these. Guthrie has recently (**2**) attempted to show that the apparent failure comes from an incomplete interpretation of the principle of substitution. He points out that according to his general theory learning to go to the lever, or to press the lever, occurs with the first approach or pressing. The contribution of his

recent paper is to suggest that the appearance of the food merely prevents the unlearning of the lever pressing act. Thus unlearning is prevented by the removal of (*CS*) and this *CS*, according to the general Guthrie theory, must be present before any unlearning can take place.

This ingenious extension or interpretation of the principle of substitution is not without its difficulties. If the food acts only by changing the situation and thus preventing unlearning, we should expect any other stimulus which changes the situation to act similarly. If, for instance, the touching of the lever whisked the rat into another cage or anæsthetized him, we should expect that the tendency to touch the lever would be similarly preserved or strengthened.

A simple integration with the emphasis on effect seems preferable to this interpretation or extension of the principle of substitution. Such an integration is suggested by Culler's description (1) of the dog with the electrode fastened to its foot. After a few whistle-shock trials the dog responds to the whistle (*CS*) by a foot lift closely resembling the original unconditioned response. In later trials, however, the dog responds to the whistle by pressing harder or by some other movement which seems to be a preparation for the inevitable shock.

We may generalize this description as follows:

A. A conditioned stimulus (*CS*), after being presented a few times will come to elicit some response (*Re*) which is characterized by its resemblance to *UR*.

B. After many presentations, *CS* will come to elicit some response (*Rn*) characterized by the fact that the combination of *Rn* with the subsequent *USn* is more adaptive than any *Rx*, *USx* combination which has previously appeared.[2]

In this statement *R* is any response to *CS*. *Re*, *Rn* and *Rx* are specific *R*'s. *US* is any unconditioned stimulus. *USn* refers to the *US* following *Rn*, *USx* to the *US* following *Rx*. *UR* is the unconditioned response to *US*. Since only the

[2] *Re* may be identical with *Rn*, may differ from it slightly or may differ from it radically. Whenever *Re* and *Rn* are identical the *A* phase, or the principle of substitution, will suffice.

*UR* to an early *US* is used, there is no need of distinguishing between various *UR*'s.

It should be noted that the effective agent in the *B* phase is the *combination* of *R* and subsequent *US*. The necessity of stressing the combination rather than the *US* alone was developed some years ago (4) and will not be repeated here.

## III. The Emphasis of the Two-Phase Principle Upon Adaptiveness

The adaptiveness demanded by the *B* phase of the principle varies for the different types of learning problems. When *US* is inevitable, the demanded *Rn* must provide for an adaptive reception of *US*. This would be an efficient reception of a valuable *US* or a bracing of the organism for a harmful *US*. When *US* is valuable and procurable, only a procuring *R* will form an adaptive combination. When *US* is noxious and avoidable it must be avoided. In the latter case, if the 'avoided stimulus' leaves some physical trace of itself (the stone crashing behind one, the pursuing animal falling behind, or frustrated at the base of a tree, the automobile shrieking in impotent rage as we dodge its attack) it would fit directly into the requirements of the *B* phase. If, however, there is no physical trace of the avoided stimulus, we must employ the previously discussed mechanism of elimination of alternative connections.

The process of learning described by the two-phase principle achieves an adaptive result much more efficiently than does the process described by either effect or substitution when taken alone. While the principle of effect insists that the end point of learning is adaptive, it suggests that the adaptation is always laboriously achieved by sheer trial-and-error. According to the principle of effect, the early appearance of an adaptive response would be the exception rather than the rule. In the case of the principle of substitution adaptation is not bound to occur. It does occur in most of the learning described by substitution, but the principle does not demand it. It is interesting to note, however, that when substitution does result in adaptation, namely, whenever *Re*

happens to be the adaptive response, that adaptation occurs very early. In such a case there is a clear advantage in the combination.

If nature could use only one of these principles she would have to chose between one principle which, in a large number of cases, achieves adaptation after a very few trials, but cannot be depended on to achieve it in every case, and a rival principle which would secure adaptation in every case but by a slow and laborious process. By using a combination of these two principles, however, she can secure the benefits of each, and by using them *in the sequence suggested by the two-phase principle*, she can achieve the ultimate in efficient adaptation. By using the two-phase principle, nature says to us, in effect, "When you need a new way of responding to a *CS*, first of all try the *UR* elicited by *US*. There are 9 chances out of 10 that this is almost the very response you need, and that a little polishing by the *B* phase will make it exactly what you want. If this doesn't turn out to be the response you need, you must rely entirely on the more laborious *B* mechanism, which ultimately will give you the required response if that response is within your repertoire." This seems the ideal combination of a mechanism which works very quickly when it does work but which does not always work, and a mechanism almost sure to work but which may take many trials.

We have said nothing about the interesting fact that the principle of substitution, which does not invoke adaptation, so often achieves adaptation. I have elsewhere (4) tried to show that this achievement of adaptation, and the consequent popularity of substitution, comes from the fact that some situations are of such biological significance that only one kind of behavior is appropriate in connection with them. For these *US*'s the only adequate anticipatory response is precisely the response which the *US* would elicit. There are some situations which demand flight. They will elicit flight after they occur. They will reinforce any flight response appearing before they occur. Any tendency leading to any other anticipatory response will be stamped out. Since in

these cases the adaptive response must always be similar to *UR*, we can describe the selection of an adaptive response in terms of similarity to *UR*. Consequently the principle of substitution, in predicting that the response resembling *UR* will be selected, also, in many cases, predicts that the only response capable of adequately anticipating *US* will be selected. We say, 'in many cases' because there are other cases in which responses not similar to *UR* will adequately anticipate *US* and it is in these cases, of course, that the principle of substitution runs into difficulties.

## IV. Toward a Single Quantitative Statement of the Two-Phase Principle

The succeeding discussion has nothing to do with the generality of the two-phase principle. It merely attempts to reduce the disjointed qualitative statement to a loose but integrated quasi-quantitative description.

The principle of effect is concerned with $\Delta c$, the change in the strength of a connection ($S$–$R$) which has functioned just prior to the occurrence of some effect. The combination of the $R$ and the effect may be rated as to biological value ($v$). This $v$ may be large and positive, large and negative or may have any intermediate value. The principle of effect states that $\Delta c = f(v)$.

It seems wise to interrupt our main argument at this point in order to clear up two matters which may disturb the reader.

(1) The expression 'a function of' is used in its most general sense. There is no suggestion that such an abstraction as biological value acts directly on a connection. The statement only means that if '$R$, Effect' combinations are arranged so that they vary systematically in biological value, they will be found to vary in their effect on just operated connections.

Presumably the thing which really acts on a connection is a pattern of neural impulses made up in part by proprioceptive stimuli from the response, and in part by exteroceptive im-

pulses from the effect. It would seem that some of these patterns of impulses strengthen just operated connections, and other patterns weaken such connections.

If we were to frame the statement of the principle of effect in terms of the agencies which actually act on connections, we should be limited, at present, to saying that some combinations strengthen and others weaken. True, we might also work out a catalogue in which the strengthening influence of each combination is listed. But such a catalogue is not available and would not be completely satisfactory if it were.

Our statement of effect is based on the assumption that surviving organisms must, on the average, have continued to encounter biologically valuable combinations. This means that the connections whose operation was followed by valuable conditions must have been strengthened while connections which led to harmful conditions must have been weakened. The biological value of a combination is thus a correlate of its properties of reinforcement and we have used it as such in our statement of effect.

(2) The statement of the principle of effect uses terms not heretofore mentioned in this discussion. The *S* is, of course, identical with the *CS* already employed. The 'effect' is frequently, but, unfortunately, not always synonomous with the *US*. Any such effect as food or pain or thwarting is likely to elicit some definite response and thus to be considered a *US*. There may, however, be a 'combination' of *R* and effect, in which the contribution of the effect is zero or almost zero. Such a 'combination' could be rated for *v* and could, consequently, fit into our statement. A very vigorous, energy consuming response, for instance, followed by no change in the environment would be considered to have negative *v*.

Since we wish the effect to be somewhat more inclusive than the concept of *US* we shall designate the effect as *AS* (after stimulus) and designate any response to it as *AR*. In this distinction *US* will be that special case of *AS* which, at the beginning of an experiment, already elicits a definite response. Other *AS*'s may either be zero and thus elicit no response, or, if not zero, may elicit not a regular, but a

sporadic or casual response, *AR*. In most cases, however, *AS–AR* is equivalent to *US–UR*.

These matters having been disposed of, we may return to the development of the quantitative statement. The reduction of the *B* phase to quantitative form has been comparatively easy. We must now state the *A* phase in quantitative terms and then attempt an integration of these two quantitative statements.

The *A* phase is called upon to explain the fact that upon the elimination of *CS–Ra*, the next response to *CS* will be *Re*, a response resembling *AR*. We can most readily describe this fact by supposing (1) that the bond *CS–Re*, like a whole host of bonds *CS–Rb*, *CS–Rc*, etc., are already in existence with a certain sub-liminal strength and (2) that the strength of *CS–Re* has been increased by the occurrence of *AR*. We should, in other words, have to suppose that the occurrence of *AR* will strengthen any unoperated potential connection which happens to lead to a response similar to *AR*.

This principle may be stated tentatively as follows:

$$Dc = f(r)$$

in which *Dc* refers to the change in strength of a connection *not recently operated* and *r* refers to the resemblance between the *R* of that connection and *AR*.

It will be seen that $\Delta c$, which refers to a just operated connection, and *Dc*, which refers to a connection not recently operated, differ from each other only in the time since the operation of the connection. This value, *t*, may range from a very small quantity to infinity.

Since $\Delta c$ and *Dc* differ only with respect to *t*, we may combine our two separate statements as follows: *At any given time*

$$\Delta c = f(v, r, t) \tag{1}$$

in which $\Delta c$ is now the change in strength of any connection whatever. This general statement should carry the following restrictions:

(2) $\Delta c \to 0$ as $t$ becomes large, $r$ being 0.

(3) $\Delta c \to 0$ as $r \to 0$, $v$ being 0.

In all but a few extremely rare instances the 'given time' refers to the occurrence of some *AS* and this, in turn, is usually a *US*. There may be 'given times' contemplated, however, in which no determinate *AS* is considered to have occurred.

Of the many permissible forms of the general expression, I should suggest one such as follows:

$$\Delta c = \frac{av}{t} + br \log t. \tag{4}$$

Expression (4) has a clear relation to the two-phase principle. We may take the '$v$' term as the principle of effect or *B* phase and the '$r$' term as the principle of substitution or *A* phase. If a connection has only recently functioned $t$ will be small, and $\Delta c$ will be affected chiefly by $v$, since a small $t$ increases the effect of $v$ and decreases that of $r$. On the other hand if a connection has not recently operated, $t$ will be large and the influence of $v$ reduced and that of $r$ enhanced.

At the beginning of the learning sequence $t$ will be large for all but one or two connections. Hence, for many connections the $r$ value will be very potent, and the increase in strength from $r$ may determine which connection will function next. In this way the $r$, or substitution, term may determine the early course of learning. In later stages of learning, however, any connection likely to operate at all has, in all probability, already operated. For all these the value of $t$ would be smaller and hence the chief influence would come from $v$, or the effect principle.

To illustrate the more detailed expectations of expression (4), let us suppose that *CS* (whistle) has elicited some response (*Ra*) and that this has been followed by *US* (shock) and *UR* ('foot lift'). Since the first response to *CS* is unlikely to escape the subsequent shock, $v$ will be negative and since the connection has just operated the influence of this negative $v$ will be great. The connection between *CS* and 'foot lift,' will on the other hand, be strengthened, since $r$ and $t$ are both large. If this process continues *CS* will cease to elicit *Ra* and come to elicit 'foot lift.' At this point, however, the value of

$t$ for the 'whistle—foot lift' connection becomes very small and hence the future strength of that connection will depend chiefly on $v$.

If, in such a situation, the electrode had been fastened to the foot of the animal, the 'foot lift' would not escape the shock. Since $v$ for 'foot lift—shock' is negative and $t$ is small, the connection 'whistle—foot lift' would be weakened and would ultimately be supplanted by some other tendency.

If the electrode is not fastened to the foot, the 'foot lift' would be followed by no definite *AS*. This combination would be considered to have a *slight* negative value. We should expect, therefore, that the 'whistle—foot lift' tendency would gradually become weakened until ultimately it would fail. Any alternative connection will, however, lead to shock and this in turn to 'foot lift.' Such a sequence will weaken the alternative connection and strengthen the 'whistle—foot lift' connection, which will be reinstated and again become gradually weakened. The sequence expected by the principle is found frequently in avoidance learning when the avoided menace leaves no physical trace of itself. The organism becomes more and more 'careless' until the menace acts again and thus reinstates the avoidance behavior.

Before any deductions from this principle can be presented in testable form we must have some means of assigning numerical values to $c$, $v$, and $r$. While there are various ways of describing the strength of a connection in numerical terms, the quantification of $v$ and $r$ is much more difficult. At present I can think of nothing better than values derived from ratings.

It may be necessary to insert an additional variable into the $br \log t$ term to reflect the possible influence of different strengths of the *AS–AR* connection. It is possible that if *AS–AR* is a well established connection such as *US–UR*, $r$ should receive more weight than if *AS–AR* is a casual connection. It may also be wise to add a small negative constant to our principle to account for the probable fact that there will be some variation in responses whenever $v$ is zero or only slightly positive.

## V. Summary

The principle of expectancy lacks generality because it fails to account for reinforcement when an undesired expectancy is not confirmed or when an unexpected but biologically valuable outcome is attained. Some hope for a completely general principle seems offered by the mutually supplementing advantages of effect and substitution. A two-phase principle incorporating these advantages reads substantially as follows:

A. A conditioned stimulus after being presented a *few* times, will come to elicit some response resembling the unconditioned response which follows the conditioned stimulus.

B. After *many* presentations the conditioned stimulus will come to elicit some response which combines adaptively with the subsequent unconditioned stimulus.

There is some possibility that this two-phase principle may be expressed in a quasi-quantitative form of which the following is a special but illustrative case:

Upon the occasion of the functioning of an unconditioned response tendency (*US–UR*),

$$\Delta c = \frac{av}{t} + br \log t,$$

in which $\Delta c$ is the change in strength of any connection (*S–R*); $v$ is the biological value of the combination of *R* and *US*; $r$ is the resemblance between *R* and *UR*; and $t$ is the time between the operation of *S–R* and the operation of *US–UR*.

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[MS. received August 14, 1941]

# MECHANISMS IN CONDITIONING

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## I. Conditioning vs. Trial-and-Error Learning

In recent years there has developed a tendency to break down the theoretical distinction between the classical notion of conditioning and that of selective or 'trial-and-error' learning. This tendency has led investigators to carry out experiments closely resembling 'problem-box' tests, treating the results so firmly under the dominance of conditioning concepts that possible relationships to the findings of problem-box studies proper may be overlooked. To be sure, Skinner (**24**) and Hilgard and Marquis (**13**) have differentiated conditioning and problem-box procedures, but they do not carry this distinction through to the basic mechanisms. We believe that closer analysis will disclose some fundamental differences of great importance.

For some time it has been recognized that contiguity in experience is the essential factor in the process of conditioning. On the other hand, in trial-and-error learning the selection process stands out clearly, and to deal adequately with this instance of learning it has been necessary to recognize not only the factor of contiguity but also the factor of motivation. Hence 'reward' and 'punishment' have figured prominently in discussions of trial-and-error learning.

It is an observed fact that when responses are accompanied by punishment they are in time less likely to come to expression, whereas those accompanied by reward are more likely to come to expression. The mechanisms responsible

for these effects on behavior are, however, not agreed upon. Thorndike's earlier notion that punishment 'stamps out' the learning and that reward 'stamps in' the learning was an attempt to explain the facts in terms of associative bonds formed only in rewarded instances. Perhaps the problem can be dealt with more adequately if it is assumed that associations are formed in both cases, a position Thorndike (**25**) now accepts. The view is that in the one case the animal learns what not to do and in the other it learns what to do,—*i.e.*, reward determines which responses will come to expression and punishment determines which will be inhibited. This point of view introduces a selection process in addition to an association process, and performance rather than the formation of associations is affected by the kind of incentive involved.

These considerations are sufficient to illustrate the fact that the particular rôles of reward and punishment have for some years constituted a most troublesome theoretical problem for investigators working with maze, problem-box, and discrimination learning.

Recent experiments (**1**, **2**, **3**, **9**, **18**, **26**) have shown that motivation may be a factor of importance in conditioning. In order to account for the facts, Culler (**6**) believes it is necessary to assume that the unconditioned stimulus has a dual function: that of giving rise to a response and that of serving as an incentive. Perhaps the recognition of the rôle of motivation in what are regarded as conditioning experiments is largely responsible for the current tendency to believe that the gap between the two forms of learning is being narrowed.

Before accepting a theoretical fusion of what may well be two distinctive forms of learning, it seems desirable to examine the evidence carefully to see whether certain of the differences between these processes may not be of basic importance. This precaution is suggested by known cases in which behavior mechanisms of qualitatively different nature present external similarities of relatively secondary importance. For instance, learning and maturation both exhibit a trend from

initially generalized response to more specific behavior, yet much ground would be lost if basic differences in mechanism were to be subordinated to this feature of similarity. Only a careful examination will show whether similarities or differences more adequately represent essential behavior mechanisms. Thus emphasis upon certain similarities may well have led investigators to overlook or to minimize some differences of primary importance between the processes of conditioning and of trial-and-error learning.

In a general treatment (21) we have analyzed the learning process in terms of concepts primarily bearing upon what

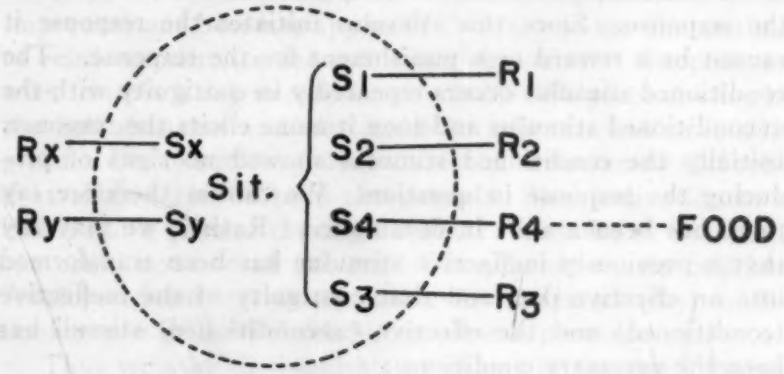


FIG. 1. Diagrammatic representation of selective learning.

occurs in the animal rather than in terms of the situation. This analysis led us to distinguish between association formation which depends upon contiguity, and selection which depends upon reward or punishment. It seems desirable to apply that distinction here, beginning with a rudimentary diagram showing how the association and selection processes operate in conditioning and problem-box situations.

In Fig. 1 the typical problem-box setting is diagrammatically represented as a situation (*sit.*) which elicits a series of responses ($R_1$, $R_2$, $\cdots$ $R_x$, $R_y$). The order in which these reactions appear may depend upon the excitatory threshold of each manner of responding, or it is possible that the responses are an expression of a tendency to vary behavior (15, 19, 20).

In any case the responses arise as parts of the repertoire that the animal brings to the experiment. Now because of the experimental set-up only one of these reactions to some aspect of *sit.* can occur in contiguity with food and so become associated with it. It is this response which gains in preference and finally dominates over other activity. When it dominates we say that the animal has mastered the problem. The animal, however, has not learned a *new* response—rather a response with a low excitatory threshold has been made dominant, or we may say it has become selected or preferred.

In conditioning the situation is quite different. Here it is the unconditioned stimulus which determines the nature of the response. Since this stimulus initiates the response it cannot be a reward or a punishment for the response. The conditioned stimulus occurs repeatedly in contiguity with the unconditioned stimulus and soon it alone elicits the response. Initially the conditioned stimulus showed no signs of producing the response in question. We cannot therefore say there has been a shift in dominance. Rather, we may say that a previously ineffective stimulus has been transformed into an effective one, and that contiguity of the ineffective (conditioned) and the effective (unconditioned) stimuli has been the necessary condition.

Once the conditioned stimulus adequately elicits the response, however, the experimental conditions may become changed. If one then presents the unconditioned stimulus after the response occurs (the method of reinforcement often employed to prevent experimental extinction), the important aspect of the problem-box procedure is duplicated. Likewise, if the conditioned stimulus calls out the critical response promptly enough, the unconditioned stimulus (*e.g.*, food; shock) will *follow* rather than precede the response. Here also the unconditioned stimulus can function as incentive even though no change in the external procedure has been introduced. In further trials the excitatory value of the conditioned response, or part of it, can thus be increased by selective learning. Since reinforcement occurs only when certain elements are present, the response may become pro-

gressively more specific, depending upon the nature of the unconditioned stimulus (*e.g.*, food or shock used as unconditioned stimuli can serve as incentives more readily than light). Thus an animal conditioned to give foot-flexion in response to a bell, through the contribution of selective learning may gradually decrease the amplitude of the flexion to the minimum necessary to clear the grill. The animal's behavior now appears to be anticipatory in nature as is the case in all selective learning. From the above we can also see why conditioned responses frequently differ from the original unconditioned response.

That the *initial* phase of the conditioning procedure is an important one becomes apparent if we try to use the method of reinforcement (or reward) from the outset. Suppose we wish to train a dog to salivate to a bell. We place the dog in the bell situation and wait until he salivates before food is given. Under such conditions it would be extremely difficult, if at all possible, to train the animal. Since salivation to a bell is not a part of the dog's regular repertoire of behavior, one could not reward the act. Once, however, the bell has developed some excitatory value for salivation (by conditioning) the method of reinforcement can proceed.

Thus we may distinguish two phases in the conditioning *procedure*, a first phase in which a neutral stimulus is given excitatory value and a second in which a selective process operates. In Pavlovian experiments attention has been concentrated upon the characteristics of the first phase although the second phase may sometimes have been present. In the first phase contiguity alone determines the nature of events. It is in the second phase that the conditioning procedure can become comparable with trial-and-error learning.

Our analysis thus leads to agreement with Culler (6) in that the unconditioned stimulus may potentially have two functions, one for determining the response, the other for motivating it. Culler points out that these different functions of the unconditioned stimulus account for two stages in the development of the conditioned response, but he does not show why the stages appear as they do. The foregoing analysis

emphasizes the fact that the successive stages grow directly out of the procedure, and makes it clear that it is the procedure rather than any intrinsic property of the unconditioned stimulus which leads to a shift in its function as training progresses.

The postulation of these two different phases in the procedure of conditioning permits us readily to integrate certain experimental findings which have seemed inconsistent with previous results, some of them among the reasons Culler has advanced for attributing a dual function to the unconditioned stimulus.

1. Culler and Mettler (7), also Girden, Mettler, Finch and Culler (12), have found that conditioned responses can be established in decorticate animals virtually as readily as in normal animals. Shurrager and Culler (23) obtained conditioning with similar ease in dogs when only a small part of the spinal cord remained in function. However, the conditioned responses formed in neurally-reduced dogs of these types are very generalized, and never reach the stage of a specific response. This seems to mean that association by contiguity is more primitive than selective learning, since it can proceed in the absence of cerebral cortex although selective learning is then excluded. The assumption that learning may take place on different neural levels is supported by experiments on conditioning under curare (10, 11). The postulation of two qualitatively different learning mechanisms makes understandable the fact that the first stage in conditioning can occur unimpaired although the second is excluded in spinal and decorticate animals.

2. Brogden (1) has found that after a conditioned response has been established a reward (food) may be substituted for the unconditioned stimulus (escape from shock) without leading to extinction of the response. If the unconditioned stimulus may acquire the rôle of incentive during the second phase of the conditioning procedure, it is to be expected that other incentives will function equivalently. That one incentive may be safely replaced by another in selective learning is a well-known fact.

3. Finch and Culler (9) and Brogden (2) found that they were able easily to develop a fifth order of conditioning although Pavlov had set the third order as the limit.

In order to accomplish this Finch and Culler first conditioned leg flexion to a tone. Since the flexion was produced by a shock grill on which the foot was placed, the animal soon escaped shock by a conditioned flexion. When this stage was reached, the selective phase could enter into the learning and prevent extinction. To establish the second order, a light-flash was presented with the tone. When the animal failed to flex the leg, shock to the thorax was administered and soon the light alone produced leg flexion. Since the tone produced leg flexion, a secondary conditioned response to light could be established and selection could then proceed as in the first order. By the time the third order of conditioning was reached it is reasonable to suppose that the leg flexion as a response to the general experimental situation was well within the animal's behavior repertoire. Under these conditions any new stimulus might call out leg flexion; hence the response was readily subject to selection and the first (or associative) stage of the conditioning procedure was probably not required. In establishing the third order, a stream of water was given with the light. Did the light in this case function as did the original unconditioned stimulus? We believe the light could have been omitted without changing the outcome. The point is that the stream of water followed by thoracic shock soon would have brought leg flexion to expression, since this response had been selected as a means for avoiding shock. If this was the case then it follows that trial-and-error learning rather than higher-order conditioning occurred in the above experiments.

The fourth and fifth orders of conditioning were then obtained with such ease that the experimenters believed they could have advanced readily to an indefinite number of higher orders. Since 'higher-order conditioning' should introduce added difficulties with each further step, the absence of such difficulties here offers further reason for believing that our analysis is a plausible one.

The same analysis would apply to Brogden's experiment (2) in which a food reward replaced the 'escape from thoracic shock' in the selective function.

There are no indications that the postulated second (selective) stage was involved in the Pavlovian experiments. This may be attributed to the use of a nonvoluntary response (salivation) and to the fact that Pavlov did not present reinforcement in a selective manner. We have thus implied that the exclusion of the selective stage would account for Pavlov's inability to carry conditioning beyond the third order. (Furthermore, the Russian experimenters had to contend with experimental extinction, since their critical response was not maintained by a selective learning.)

4. Brogden, Lipman and Culler (3) conditioned guinea pigs to run in a rotator in response to a 1000-cycle tone. Shock was used as the unconditioned stimulus. In one group the running shut off the shock and so afforded relief, in another group the response was not thus rewarded.

During the first 75 to 100 trials the two groups showed essentially identical progress, the running in response to the buzzer being part of the pattern in about twenty per cent of the trials. After 100 trials, however, the two groups progressed quite differently. The group that was rewarded for running responded by running one hundred per cent of the time, after an average of 206 trials; the group not rewarded in this manner showed questionable increase in the tendency to respond by running. Instead, the animals of the latter group held their breath and tensed their body musculature. The experimenters believed that the group which responded to the tone by running duplicated the unconditioned response, whereas the other group showed anticipatory behavior.

It seems reasonable, however, to regard holding the breath, tensing the muscles, and leaping forward all as parts of the unconditioned response to shock. Since tone was coupled with shock, it could call out any part or all of this pattern. The experimenters measured only the running and this aspect was equally shown by both groups during what we regard as the associative stage.

The one group, however, was rewarded for running and, as a consequence, it is to be expected that this aspect of the response would become more and more selected. The other group may have minimized the shock effects by tensing the musculature (the experimenters considered this to be the case), and if this is true this aspect of the pattern should become selected. If the tensing did not relieve the shock effects then we should expect the response to tone to remain generalized. (The data are not sufficiently detailed to determine which of these alternatives occurred.)

In any case the final response was distinctly different in the two groups and we believe the second or selective stage to be responsible for the difference. The experimenters believe the form of the incentive resulted in two contrary forms of conditioning. Since selective learning is an established fact, it seems unnecessary to postulate two forms of conditioning to account for the findings.

5. Kellogg (**14**) emphasizes the presence of individual differences in the form of the conditioned leg flexion. From the types of this response which occur in different dogs he believes the notions of 'stimulus substitution' and 'anticipatory responses' in conditioning can both be shown. From our analysis Kellogg's results are to be expected. Individual differences in selective learning are the rule, since each animal selectively learns from its own repertoire. Because Kellogg's dogs were able to avoid shock by flexing the leg we believe the marked individual differences observed were due to the *selective learning* occurring after the first stage.

6. Loucks (**18**) found that the dog could not be conditioned to a buzzer when shock to the motor area of the cortex was used as the unconditioned stimulus for leg flexion. This indicates that the unconditioned stimulus must have effective sensory relations as well as a purely motor function, since only the sensory aspect of the unconditioned stimulus was lacking in this experiment. However, a change in procedure entirely altered the outcome of the experiment. When the animal was given a food reward each time leg flexion occurred, positive results were obtained.

Loucks contends that motivation made possible the establishment of a conditioned response otherwise unobtainable. According to our analysis conditioning did not occur in this experiment. Rather, cortical shock in the buzzer situation elicited a response, and reward was able to function selectively. Under these conditions foreleg flexion became associated with reward just as string-pulling becomes associated with reward in the problem-box situation. The first stage, *i.e.*, the association between a neutral stimulus and the experience of the unconditioned stimulus, was thus omitted. Since selective learning is facilitated when an animal is 'put through' a given act, it is not surprising that Loucks was able to obtain selective learning under conditions in which cortical shock offered a very effective way of manipulating the desired response.

That this distinction is real and not merely verbal in nature can be demonstrated through the fact that the two interpretations lead to opposite conclusions in certain critical instances.

Crisler (5) and Light and Gantt (17) have shown that conditioning can occur without the animal making the critical response during training. It is merely necessary to present the conditioned and unconditioned stimuli in contiguity. Thus, Light and Gantt prevented the unconditioned stimulus from producing a response by crushing the motor roots of the essential spinal nerves. On regeneration of the destroyed motor pathways it was found that the buzzer, which had been paired with the shock to leg (unconditioned stimulus), now produced leg flexion. This result could not have occurred had not the training somehow established an association between the buzzer and shock experiences. It thus follows that making the critical response is not essential for the development of true conditioning.

In trial-and-error learning, however, actively making the response is very important, since the critical action must come to expression if it is to be selected. Thus, if our contention that Loucks' experiment involved trial-and-error learning is correct, it follows that negative results would be obtained if

the Loucks' experiment were repeated with the motor nerves crushed but using buzzer and cortical shock as he did. If, however, it is claimed that ordinary conditioning occurred, this modification should not affect the findings, since making the response is not essential to conditioning.

## II. Sensory-Sensory vs. Sensory-Motor Connections

It appears to us that much of the confusion in the interpretation of conditioning data arises from a failure to analyze what has happened to the animal. As a rule, interpretations are based on what the animal *does*. Behind the animal's behavior are the mechanisms responsible for the behavior, and

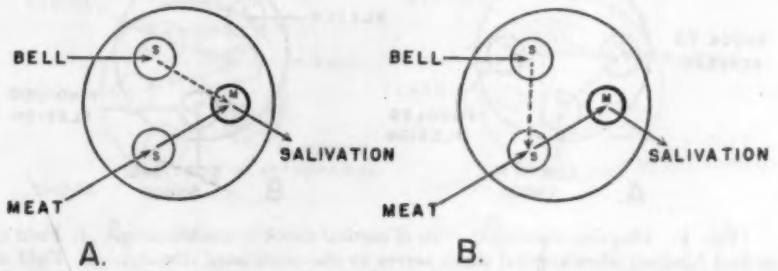


Fig. 2. Diagrammatic representation of two interpretations of the conditioning process. The large circles indicate the field of cortical function; the small circles indicate sensory and motor regions. *A*. Conditioning viewed as a *substitution* process. *B*. Conditioning viewed as an *extension* process.

an improved understanding of the mechanisms should throw needed light on the overt occurrences. The point can be nicely demonstrated in connection with an extension of our preceding discussion.

In conditioning, we are told, the conditioned stimulus becomes a *substitute* for the unconditioned stimulus, which apparently means that both occupy similar functional positions once training is effective. This generally accepted view is graphically presented in Fig. 2-*A*. The dotted line indicates the functional connection which is assumed to arise through training.

An obvious alternative suggested by the traditional doctrine of the association of ideas is indicated in Fig. 2-*B*.

Here the dotted line suggests a very different functional connection. The first diagram indicates that a *sensory-motor* connection is established, whereas the second offers a *sensory-sensory* connection as the essential dynamic change. It is the latter which Maier and Schneirla (**21**, Chap. XV) have regarded as most probable.[1]

In the first case the term 'substitution' is a pertinent description of the postulated change, but in the second this term is misleading. Rather the term *extension* is more appropriate, as indicating the enlargement of a sensory pattern controlling the critical response. Although the second

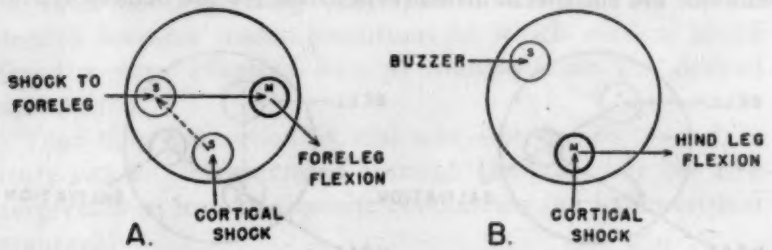


FIG. 3. Diagram showing the rôle of cortical shock in conditioning. *A.* Field of cortical function when cortical shock serves as the *conditioned* stimulus. *B.* Field of cortical function when cortical shock serves as the *unconditioned* stimulus.

alternative is seldom considered it seems to best fit the facts. Let us consider some experimental findings which bear significantly on this question.

Loucks (**18**) found that cortical shock to the motor area could function as a conditioned stimulus, but *not* as an unconditioned stimulus. If conditioning depends upon an association between two sensory cortical patterns, then it is clear why cortical shock is thus limited in its functions. In Fig. 3-*A* the dotted line represents our conception of the new dynamic relationship which is established when cortical shock is the conditioned stimulus. Shock to the leg furnishes the sensory component arousing motor cortex, which in turn

[1] The implications of this question extend far beyond the field of mammalian conditioning. Thus we have based our theoretical comparison of psychological capacities among the lower vertebrate classes—and in fact among the higher invertebrate groups as well—(**21**, Chaps. V–X) upon *sensory integration* as the process critical for the qualitative nature of behavior.

elicits the unconditioned defense reaction. When cortical shock is introduced as unconditioned stimulus, however, the activated cortex directly gives rise to leg flexion and its function is then purely motor. Fig. 3-*B* shows that a sensory component is absent in this experiment, and since negative results were obtained when a buzzer was used as the conditioned stimulus, we are led to regard the sensory aspect of the *unconditioned* stimulus as essential for positive results. If conditioned responses depend upon a sensory-motor connection, it is difficult to account for these negative findings. (We have already shown how the addition of reward can transform the negative to positive results.)

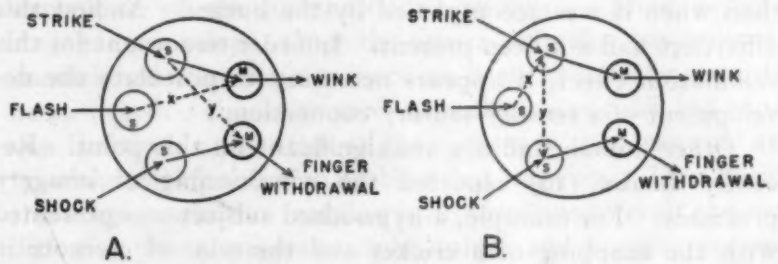


FIG. 4. Comparison of *sensory-motor* and *sensory-sensory* views as applied to Shipley's experiment. In *A*, the acquired connections, *x* and *y*, do not account for the association of 'flash' and 'finger withdrawal' functions. In *B*, the acquired connections, *x* and *y*, effectively account for the association of these cortical functions.

The outcome of an experiment by Shipley (**22**) also favors the sensory-sensory interpretation. He first conditioned a wink response to a light flash by presenting the light with a strike below the eye (*US*). Next he conditioned finger withdrawal to the same strike below the eye by presenting the strike (*CS*) with shock to finger (*US*). The crucial test was now to reintroduce the light flash. In nine of his fifteen human subjects finger-withdrawal was obtained with the flash, which had never been paired with shock to finger. If we postulate a sensory-motor connection as the essential relationship, the system is like that sketched in Fig. 4-*A*. But the acquired connections involved here, *x* and *y*, provide no direct route between the cortical components of flash and finger-withdrawal, and it is necessary to postulate highly

questionable backward-running associations or kinesthetic components to account for the findings. If, however, we assume that conditioning develops sensory-sensory functional relations, the system is like that diagrammed in Fig. 4-*B*. Here the acquired associative relationships *x* and *y* furnish a direct functional connection between the cortical pattern set up by the flash and that controlling the finger-withdrawal. Apparently Shipley did not consider this alternative.

In an experiment by Cason (4) on the conditioned wink response, a sound was paired with a shock below the eye. After this training the subjects reported that the shock when presented alone seemed weaker ('and lacking in something') than when it was accompanied by the buzzer. At first this difference had not been present. In order to account for this summation effect, it appears necessary to postulate the development of a sensory-sensory connection.

Other human findings are significant on this point. Recently Leuba (16) reported the conditioning of imagery processes. For example, a hypnotized subject was presented with the snapping of a cricket and the odor of creosote in combination. When awakened and presented with the snapping of the cricket, the subject was caused to sniff and report a peculiar odor. In this case also, the assumption of a controlling cortical pattern centering around a sensory-sensory connection serves to account most adequately for the results.[2]

## III. Summary and Conclusions

For some time the problem of the relation between conditioning and selective (trial-and-error) learning has been a source of controversy in the study of learning. Because the conditioning phenomenon can be studied conveniently and described in relatively simple terms the possibility of reducing

[2] It may be asked why it was necessary to use hypnotized subjects to produce this effect. The answer seems to be that since implicitly aroused images are readily distinguished by the waking subject from direct perceptions, the imagery process is readily suppressed and not reported. Hence the *response* aspect of conditioning affords much better evidence of training and accordingly is commonly emphasized in the literature. However, the significant fact that normal subjects may anticipate the unconditioned stimulus supports our view that the counterpart of Leuba's sensory pattern also exists in the conditioning of waking individuals.

all learning to conditioning has been an attractive one. Recent evidence on the apparent function of motivation in conditioning has tended to broaden the conditioning concept so that the differences between selective learning and conditioning seem to have been broken down or at least substantially reduced. In this paper we have offered some cogent reasons for retaining the distinction.

Our analysis has served to point out that in the *first stage* of the conditioning *procedure* a neutral stimulus develops excitatory value for a response it previously did not control. The essential condition for this change in the animal is contiguity between the experience of the neutral stimulus and the experience of the unconditioned stimulus. Then a *second stage* may develop. Once the neutral stimulus has become a conditioned stimulus, the response it elicits may be rewarded or punished. Unconditioned stimuli such as food and shock can function in this stage in the same way that 'reward' and 'punishment' function in problem-box learning. Thus during this stage a selective learning is involved; as a result the conditioning procedure ceases to be unique and assumes the psychological characteristics of trial-and-error learning. Then, as in all selective learning, the critical response becomes more and more specific the more effectively it leads to reward or to escape from punishment.

Before a response can become selected, it must be in the animal's repertoire. It is characteristic of Pavlovian conditioning procedure to utilize a response which is not likely to be available in a given situation unless its unconditioned stimulus is present. However, in problem-box and other trial-and-error situations, the critical response is available in the situation and a specific unconditioned stimulus is not required to bring it to expression.

Thus selective learning depends upon reward and punishment as well as on contiguity, whereas associative learning requires only contiguity in experience. Failure to distinguish these as qualitatively different forms of learning leads to inconsistencies, since it results in overlooking the possibility that different learning situations may involve the two mecha-

nisms in different ways. We have pointed out that the conditioning procedure first introduces purely associative learning, but then may admit selective learning depending upon certain special conditions. The principal conditions determining this change concern a possible function of the unconditioned stimulus as incentive and probably also the degree to which the critical response is voluntary and thus subject to selection. If these conditions are fulfilled, the conditioning procedure frequently used may result in a pattern which has the characteristics of both associative and selective learning.

Since the first stage of the conditioning procedure is unique in requiring an unconditioned stimulus, we believe that the term 'conditioning' should be restricted to the changes occurring during this stage. It is the resulting limited routine type of response which Pavlov originally designated as 'conditioned.' Performing tests which are in reality problem-box experiments and terming the process 'conditioning' does not reduce problem-solving to conditioning, but merely confuses the issue.

Restricting the term 'conditioning' in the above sense serves to focus attention upon the nature of the associative process. We have offered evidence and considerations favoring the view that the essential change, as studied in conditioning, involves a new dynamic relationship between the sensory cortical pattern of the conditioned stimulus and that of the unconditioned stimulus. Thus we maintain that the key process is a modified sensory integration. The commonly accepted view of conditioning as depending upon a new sensory-motor connection seems inconsistent with experimental findings. Rather than speak of conditioning as 'stimulus substitution' it is more advisable to regard it as the extension or qualitative enlargement of the pattern of stimulation which will elicit the critical response.[3]

[3] Comment by Dr. Maier on Dr. Stephens' paper, "Expectancy vs. effect—Substitution As a General Principle of Reinforcement," appearing in this issue:

The latter portion of Dr. Stephens' paper shows that he has been confronted with the same problem with which we have been primarily concerned. His solution also

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shows some similarities to ours. The basic difference in my estimation is that Stephens' paper represents an attempt to reduce (or elevate) conditioning to trial-and-error learning, whereas our paper attempts to reduce only the latter portion of conditioning (the stage at which reinforcement occurs) to trial-and-error learning. Since he does not break the conditioning procedure into two phases, his interests may turn to a quantitative aspect of the problem. We emphasize a qualitative stage and cannot deal with quantitative aspects until the qualitative stages have been distinctly separated. The stages being qualitatively different demand quite different units of measurement.

That our first stage (association by contiguity) is a real one is supported by Stephens' attempt to explain the conditioned leg flexion in which the leg flexion does not achieve avoidance of shock. According to his analysis such conditioning procedure would lead to a tendency other than leg flexion.

The very fact that this procedure produces responses *simulating* the unconditioned response lead to the development of the notion of conditioning. Only recently has the emphasis been placed upon certain differences between the conditioned and the unconditioned responses. The differences have been explained by regarding the conditioned responses to be anticipatory in nature. Stephens goes a step further in differentiating the two responses and in so doing fails to account for the similarities.

Stephens' criticism of expectancy when used as an explanatory principle is very convincing. It seems that our sensory-sensory integration accounts for instances in which anticipatory behavior is apparent and at the same time does not require the concept of expectancy to explain it.

It also seems that Stephens' analysis of selection would be more convincing if he clearly distinguished between conditioning and problem-box situations. To regard food as an unconditioned stimulus in a problem-box situation in which a string must be pulled so broadens the meaning of the term that its basic connotation is lost.

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[MS. received August 25, 1941]

# A RESTRICTED EXTENSION OF FECHNER'S LAW FROM SENSATION TO BEHAVIOR

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Although the theory of Fechner that sensation varies arithmetically as the stimulus varies in geometrical ratio is recognized as only an approximation, it also appears that the approximation is quite close over a considerable range of the variable of retinal illumination (9). Furthermore, the range of reasonable applicability of the Fechner law encompasses precisely those brightnesses under which critical visual tasks are usually performed. Hence the failure of the law for scotopic stimuli or for extremely high brightnesses does not lessen its practical significance in many realms of lighting and of seeing (2). For example, this law is basically important in the formulation of a gradated series of illumination-levels upon the rational grounds of intrinsic visual effectiveness (3). In view of these considerations, a valid extension of the Fechner law from sensation to behavior and response, even if only approximate, would add to its significance and usefulness in practice.

The possibility of extension of the Fechner law is indicated by the fact that certain psychophysiological effects of critical seeing also vary arithmetically as the level of illumination under which the seeing is done varies in geometric ratio. Moreover, it is significant that this relationship is applicable to diverse physiological responses of the individual to changes in illumination under which the task is being performed. The present discussion pertains chiefly to two such effects—*viz.*, the development of nervous muscular tension and the augmentation of the rate of blinking while reading under various intensities of illumination. It will be noted that both of these muscular responses are involuntary in character although the

muscles involved belong to the voluntary system. It is also probable that they are not initiated directly by photopic stimulation *per se* but rather that they arise indirectly as a result of differences in 'ease of seeing.' In both these respects, these muscular responses differ from those involved in the pupillary light reflex.

*Nervous muscular tension.*—It is a matter of everyday experience that nervous tension and fatigue are usually accompanied by a tightening of certain muscles. Hence it is conceivable that appropriate measurements of 'tenseness' might constitute a summation of subtle psychophysiological reactions to light, lighting and other factors which influence the performance of visual tasks. Our technique (4) for appraising the relative degree of tenseness developed while reading is to record the changes in muscular tension of the fingers of the left hand. This was accomplished by resting the fingers naturally and comfortably upon a large flat knob which was mechanically attached to apparatus suitable for measuring the pressure exerted upon the knob which was ostensibly provided for recording the pages of the text being read. The recording mechanism was concealed from the subject, and during the period of a year in which the investigation was in progress, the subjects were unaware of the fact that the pressure they unknowingly exerted upon this device was being recorded.

The subjects were instructed to read at their natural rates during 30-minute test-periods conducted under each of three levels of illumination—1, 10 and 100 footcandles, respectively. The experimental results are summarized in Fig. 1 for each of the 14 subjects and for the entire group (4). It is obvious from these data that the psychophysiological effects of reading under various levels of illumination are of such definiteness and magnitude that they are capable of creating muscular changes which are measurable by the method described. Furthermore, it will be noted that *all* of the subjects became less tense as the illumination was increased, and particularly, that the degree of tenseness varied approximately with the logarithm of the illumination over a range from 1 to 100 footcandles. The

quantitative character of the latter conclusion appears warranted in view of the fact that the relative probable errors of the average values of indicated nervous muscular tension (broken-line, Fig. 1) are less than 1 per cent. A detailed

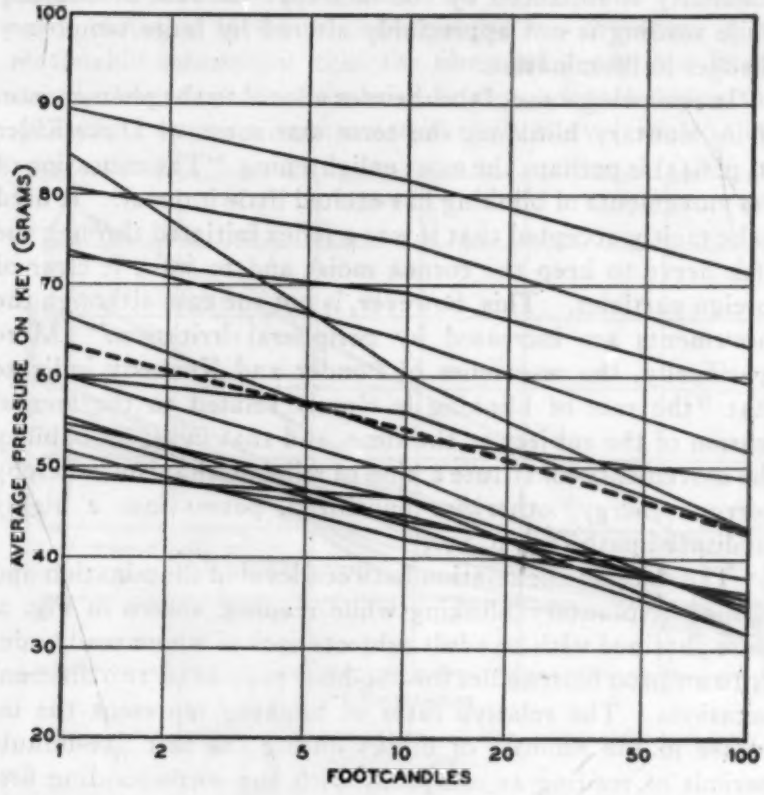


FIG. 1. These relationships were determined from 10 tests under each level of illumination by each subject. The individual probable errors vary from about 1.5 to 4.0 per cent.

statistical analysis of the data of the individual subjects is available elsewhere (4).

*Rate of involuntary blinking.*—A consideration of the possible causes of involuntary blinking leads to the theory that these movements of the eyelids are related to the degree of 'tension' or state of 'fatigue' of the subject which exists at the time. Since it has been observed that the frequency of blink-

ing is fairly constant for the individual under constant experimental conditions, it is possible for the rate of blinking to serve as a useful and practicable criterion of the visual effort expanded in seeing under different levels of illumination. This possibility is enhanced by the fact that the rate of blinking while reading is not appreciably altered by large temporary changes in illumination.

In reviewing some of the theories related to the phenomenon of involuntary blinking, the terse statement of Duke-Elder (1, p. 645) is perhaps the most enlightening: "The causation of the movements of blinking has excited little interest. It used to be tacitly accepted that it was a reflex initiated through the fifth nerve to keep the cornea moist and to wash it clear of foreign particles. This, however, is not the case although the movements are increased by peripheral irritation." More specifically, the researches of Ponder and Kennedy indicate that "the rate of blinking is closely related to the mental tension of the subject at the time, and that in all probability the movements constitute a kind of relief mechanism, whereby nervous energy, otherwise unutilized, passes into a highly facilitated path" (7, p. 89).

The data on the relation between level of illumination and rate of involuntary blinking while reading, shown in Fig. 2, were obtained with 10 adult subjects each of whom read under 1, 10 and 100 footcandles for one-hour periods on two different occasions. The relative rates of blinking represent the increase in the number of blinks during the last five-minute periods of reading as compared with the corresponding first five-minute periods. It will be noted that the rate of blinking increases with time of reading and that this increase diminishes with increased illumination for *all* subjects. Furthermore, it will be noted from the average relationship for all subjects that the increment in rate of blinking decreases approximately linearly with the logarithm of the illumination. This conclusion appears justifiable, from a statistical viewpoint, since the relative probable errors of the average data are 2.9, 2.5 and 1.0 per cent, respectively, for levels of illumination of 1, 10 and 100 footcandles. In comparing the average experimentally

determined relationships between nervous tension and illumination and between rate of involuntary blinking and illumination, it is of interest to note that the departures from linearity are opposite in direction in the two cases and comparable in magnitude with the probable errors of the data. Since the same variation in the visual stimulus was involved, it is a reasonable assumption that the observed departures from a strictly linear relationship arise from accidental rather than systematic errors. In any case, there is no evidence of a lack

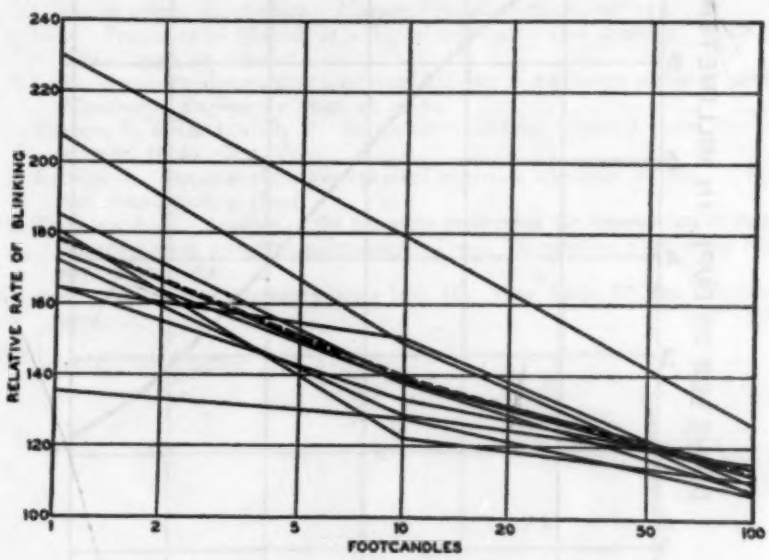


FIG. 2. These relationships were determined from 2 tests under each level of illumination.

of linearity on the afferent side of the reflex arc. Such a conclusion is also consistent with the arithmetical changes in pupil-size resulting from geometrical changes in illumination over a similar range of stimulus-intensity (8), as shown in Fig. 3, for intensities between 1 and 100 footcandles. This fact is particularly significant since the pupillary changes are initiated directly by changes in the stimulus.

*Summary.*—In view of these and other concordant relationships between intensity of illumination and involuntary muscular responses of diverse character, it appears that a rather

definite empirical basis exists for an extension of Fechner's law from sensation to behavior. Specifically, it might be stated that the psychophysiological effects of more or less prolonged and critical visual effort vary arithmetically as the visual

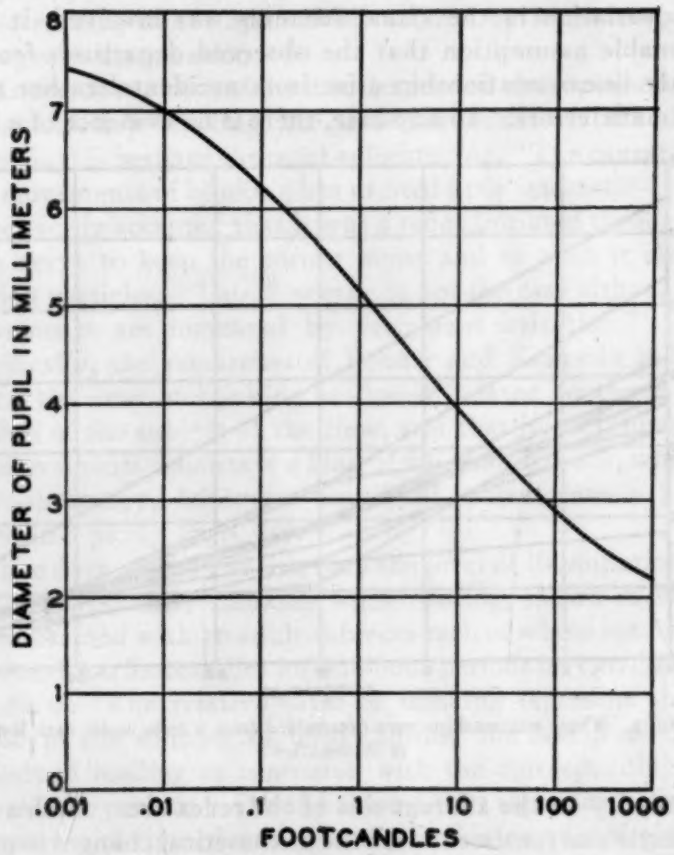


FIG. 3. The size of the pupil after 15 minutes exposure to various levels of illumination of a 'white' surface of 80 per cent reflection-factor (After Reeves).

stimulus (intensity of illumination) varies in geometric ratio over a rather wide but limited range. Within these limitations, it appears that the Fechnerian logarithmic relationship is not appreciably distorted, in cases of intensity transmission, by being reflexly transformed into motor response of an involun-

tary character. The locus of this logarithmic relationship, according to Troland, (8) is between the stimulus and the nerve conduction process.

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[MS. received August 7, 1941]

# SOME DANGERS IN THE USE OF SYMBOLIC LOGIC IN PSYCHOLOGY[1]

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Judging by current literature,[2] theoretical psychology in America has become pretty thoroughly convinced of the value of symbolic logic. As a philosopher I cannot conceal my pleasure over this recognition of the importance to psychology of one of the disciplines which by tradition is included in philosophy. My own conviction is that we are suffering today from too great a separation of the specialized research of the sciences and the more general, synthetic inquiry of philosophy, and that any sincere attempt to bring understanding and coöperation between these two interests is to be wholeheartedly encouraged. So if I point out what seem to me to be certain dangers in the current trend in psychology, it is not because I wish to disparage it, but rather because I fear that, if it grows too rapidly, its roots will be too shallow, and it will die almost as quickly as it has grown, to be replaced, perhaps, by an even greater suspicion of everything philosophical than that which it had to overcome. The dangers I shall indicate may be said to be all 'psychological,' but surely no psychologist would add 'merely' to this characterization (though I fear some logical positivists might). For, they are real dangers for which psychology, as a human enterprise, must be on the alert. They are, however, dangers, not insuperable obstacles.

The recognition and appraisal of these dangers in the use of symbolic logic in current psychology (or, as I shall call it, the 'logicizing of psychology') may perhaps be best introduced by

[1] Developed from a talk given to the Psychology Seminar at Stanford University on May 26, 1941, and read before the Philosophical Club of Stanford University on August 2, 1941.

[2] See, *e.g.*, bibliographies in (44, 26).

a brief word concerning the historical development of symbolic logic. I should like to divide this development (with some torturing of fact) into two main phases, which I shall characterize as the 'formative' and the 'reformative' periods, the difference being one of general attitude toward symbolic logic.

The formative period was one of essentially internal development. It culminated in Whitehead and Russell's *Principia Mathematica* (1910–1913). Two of its basic goals were: to make logic a simpler and more powerful calculative machine, and to apprehend more clearly the universal nature of strict deduction. These two endeavors started, to a great extent, separately. The first or 'calculative' interest was the main motive in the mathematicizing of logic, that is to say in the development of an 'algebra of logic.'[3] It started as a frank imitation in logic of the powerful calculative methods of algebra (using equations, variables, operations analogous to those in algebra, etc.). Its great weakness at first was that it was a class-calculus. Thus its own procedures of calculation, its principles of deduction, were omitted (since they are propositions, not classes). This was corrected in *Principia Mathematica*, which derives the logic of classes from a propositional logic.

The second or 'apprehensive' interest, though not the sole motive, seems to have been definitely present in the logicizing of mathematics, or at least to have grown naturally out of this development.[4] This movement started with the attempt to carry farther the simplification of the theory of number, which had already reduced numbers to natural numbers, by deriving natural numbers from a few logical (non-numerical) primitive ideas and propositions. It culminated, in *Principia Mathematica*, in the attempt to derive all basic mathematical concepts from a few logical ideas and postulates by means of a few simple deductive procedures, themselves clearly formulated and contained in the total system. Thus the two great branches of pure deductive thought, logic and mathematics,

[3] George Boole, writing about the middle of the 19th Century, was perhaps the most important representative historically (4, 5). For a general survey of the formative period, see (30, 31, 39).

[4] G. Peano was a key figure historically (33).

were united, and their common presuppositions stated and simplified. The outcome was the recognition, now well-nigh universal, that all rigorous deduction is tautological; valid conclusions assert nothing not already asserted in their premises.

Since the publication of *Principia Mathematica* there have been many internal developments in symbolic logic,[5] but the main constructive work was completed in that book. By and large, the symbolism and procedures of *Principia Mathematica*, also its basic, if largely implicit, theoretical result (that rigorous deduction is tautological) have been accepted. The general interest in symbolic logic has shifted from formation to reformation, from internal development to application in the various enterprsies aimed at gaining knowledge of the world, an application that seeks to reform traditional methods. This reform movement is mainly the work of the so-called logical positivists.[6] At first this reform was ostensibly directed toward philosophy (really, the disciplines in philosophy outside logic). This is strikingly the case with Wittgenstein (47), though in fairness we should treat him as a transitional figure, for he is important in the explication of the tautological nature of deduction. Since proof is wholly tautological, he argues, it can give us no knowledge of the world. Knowledge must be wholly based on observation of particular matters of fact, *i.e.*, on the empirical sciences. Hence philosophy can say nothing about the world; it should keep still or confine itself to making our ideas clear. Carnap (9, 10) puts it somewhat more prosaically by saying that philosophy should confine itself to the analysis of the syntax of the language of science, *i.e.*, to the formulation of the rules of the formation and transformation (translation) of sentences in the language of science. With one major exception, Carnap, like Wittgenstein, restrains his reformative zeal to philosophy. In fact, he really restrains himself more severely. One result of Wittgenstein's position

[5] Witness multi-valued logics, the matrix method, etc. Since this article was written, the author, largely through the influence of G. Bergmann, has developed a greater appreciation of the purely theoretical developments in symbolic logic since the publication of *Principia Mathematica*. This has not led hem, however, to reject his main contention, *viz.*, that in the logicizing of psychology the main spirit has been one of reform.

[6] For a good general summary and criticism, see (45).

was the denial of any cognitive value in induction, generalization, or prediction: knowledge is restricted to particular observations of particular facts. The implications of this denial would involve a tremendous reform of scientific methodology, amounting, I believe, to an annihilation of modern science. This disquieting result led Carnap to substitute verifiability for actual observation (**9**, **10**), and later confirmation (approximation to verification) for verifiability (**11**). Reichenbach (**35**) has carried this same tendency farther in his concepts of weight and probability, and largely for the same reason, namely, to avoid reforming the sciences into nonexistence (which avoidance in the case of Reichenbach has the interesting consequence of allowing the traditional philosophical disciplines to creep back into respectability). The one major exception in the restriction of Carnap's reformative zeal to philosophy is his advocacy of a unified language of science, or 'physicalism' (**8**, **9**). Though he claims to confine himself here to a mere statement about the languages of science, *viz.*, that only the physical language is such that all scientific statements occur in it or can be translated into it, yet it seems clear that he is *advocating* the use of this language as *best*, since it alone can unify science.[7]

More recently, logical positivism has frankly extended its reformative ardor to scientific methodology. This seems clear in the program of monographs in the *International Encyclopedia of Unified Science*. Its effects are abundantly clear in the field of psychological methodology. We can readily recognize here the two basic marks of this reformative movement. On the one hand, there is the insistence that theoretical deductions be reduced to strict tautologies, and that, in doing so, the primitive elements (undefined terms and postulates) be clearly indicated. Good examples of this emphasis upon 'postulational techniques' are to be found in Clark Hull's systems (**19**, **20**, **22**). On the other hand, there is the insistence that, apart from mere devices of deduction not appearing in the conclusions, all concepts and assertions must be

[7] Bergmann (**1**) has indicated that there was a reformative tendency in the early fervid advocacy, in physicalism, of what was supposedly a simply linguistic desideratum.

observationally definable and verifiable. This emphasis originated, independently of logical positivism, in the demand for wholly operational definitions, stemming from Bridgman (6), and championed in psychology by many, *e.g.*, by S. S. Stevens (41, 42, 44). But that the use of postulational techniques and the demand for operational definitions in psychology are or at least can and should be integral aspects of a common movement has been frequently urged, particularly by those who have felt the influence of logical positivism (34, 2, 28, 44). Therefore I feel justified in referring to both by the single phrase, 'the logicizing of psychology.' The chief actual difference has been, I judge, that operationism has not emphasized deductive rigor, the need of theoretical development, as much as has 'postulationism,' and has, in one sense, emphasized empirical observation more, *viz.*, by refusing to admit any observational data in science except observations of operations.[8]

It should be obvious that it is symbolic logic in its reformative, rather than in its formative, phase which I take to be making such a great impression upon contemporary psychology. The logicizing of psychology is carried on in quite a different spirit from the earlier logicizing of mathematics. It is possible that a new Whitehead and Russell will appear and write a *Principia Psychologica*, showing that all the basic psychological concepts in actual currency (and especially those not susceptible of mathematical formulation) can be derived from a small set of primitive elements. But the signs of the times are not favorable. Apparently psychology is not

[8] Bridgman insists (7) that operationalism is nothing new, institutes no reform in science, is, in fact, just an insistence upon honest report of what the scientist actually does. Stevens says, ". . . operationism seeks only to discover how scientists do what they do" (44, p. 233). Yet it seems obvious that operationism conceives itself to be a reform movement in science, clearing away verbal and metaphysical 'non-sense.' The contradiction can, perhaps, be eliminated by supposing 'scientist' to be ambiguous, the operationists simply *report* the procedures of their ideal or 'true' scientist; they seek to *reform* the procedures of actual scientists. But even on the basis of 'true' science, there seems to be a difference between the observational requirements in the use of postulational techniques or, more generally, the program of logical positivism, on the one hand, and the demands of operationism, on the other. For the former, any perceptual data, any 'protocols,' are satisfactory material for verifying theoretical conclusions; for the latter, only operations are admissible. It is the contention that perceptual data other than operations are not to be used in the definitions of scientific concepts that makes operationism a *distinctive* form of scientific positivism.

satisfied with itself. It asserts it is a science, but has serious subconscious doubts. It has amassed a tremendous body of observational and experimental data, much of which is of great value. But it has been unsuccessful in attaining an acceptable theoretical unification of this material. We do not find in psychology (as logic, in its formative period, found in mathematics) an approved body of theory ready for logical analysis and reduction. Psychology is in search of such. It is ripe for reform. Not long since it was statistical and correlational techniques that were to elevate it from clerical tabulation to a strict science. Now it is operational definitions and postulational techniques that are to do the trick. But how are they to do the trick? What are the reforms in theoretical psychology that are to be instituted by the logicizing of psychology?[9]

First, symbolic logic is to increase the clarity and decrease the ambiguities of psychological theory and terminology. This is to be attained through explicit formulation of all assumptions (**22**, p. 10) and the formal definition of all terms except a small set of undefined terms used in the definition of the others, the definition of all formally undefined terms operationally, *i.e.*, by denoting the operations to which they refer (**42**, p. 323), and the avoidance of terms highly ambiguous in popular usage by the adoption of non-linguistic symbols (**48**, pp. 1, 2).

This aim of clarity is surely an excellent one. Popular literary style in psychological expositions, on the one hand, and, on the other, vehement advocacy of some particular psychological 'ism' through its special jargon, could both profit by any serious attempt to gain clarity. I would simply warn that there is no automatic technique that can be substituted for eternal vigilance in the attainment of it. Also, I would urge that there are other fine aims, some of which might have to be sacrificed by a too strenuous attempt to attain this one. The first of these warnings is directed not merely toward the real possibility of ambiguities in non-linguistic symbols in

[9] For a summary statement especially of the values of postulational techniques in psychology, see (**32**).

general, but even more specifically toward the possibility of ambiguity in formally undefined terms. Suppose we attempt to give operational definitions of these formally undefined terms. Any such attempt would result in defining any single term by a set of particular, actual operations, supposedly similar in certain respects.[10] Such a set would include dissimilarities; in fact, each particular operation in it would be unique. Consequently any term defined by operations would be ambiguous. It would refer to any operation sufficiently similar to a given operation or operations. But 'sufficiently' would be left operationally undefined, as would also the respect in which there must be sufficient similarity. This ambiguity could be completely avoided only by the heroic but impossible procedure of correlating each formally undefined term with a single, particular, actual operation. But even the less heroic measure of defining by a small set of particular operations would give a set of formally undefined terms, in any theory of any scientific significance, so formidable as to be unmanageable.

Even suppose, however, this source of ambiguity conquered, there is another. For, actual operations do not enter theory, certainly not symbolized theory, in *propria persona*. They enter only by proxy, by symbols of them.[11] They are described. Such a symbolic description of a set of particular operations must (to be read and understood, to say nothing of being written down in the first place) omit reference to an indefinitely large number of properties of the set, on the basis that these properties are irrelevant. And many of these properties are similarities in the set. *E.g.*, a psychologist will usually omit, from the description of the operations he reports, all reference to his own characteristics as experimenter, peculiarities of his laboratory, of his various subjects, etc.

[10] A definition in terms of certain similarities of a set of operations (as contrasted with the set itself) would, on the operational criterion, require another set of particular operations to denote unambiguously the similarities meant.

[11] This is the basis of Carnap's contention that the study of the logical syntax of the language of science can completely disregard the subject matter (the objects) of the science (9). *E.g.*, it can distinguish 'protocol sentences' though completely ignoring 'protocol experiences.'

Now it frequently [12] happens that such undescribed properties are necessary for the occurrence of the sort of operations being described. Furthermore, even as regards the properties symbolically described, there is a large opportunity for ambiguity. Ordinarily these are described in linguistic symbols, with all their connotative ambiguity. In some instances diagrams, photographs, and other devices can be used, but the alternatives here are limited by the fact that conventional symbols, not formally defined in the system itself, must be used, and thus import into the system the vagaries of social usage. Where the formally undefined terms are not operationally defined, there is an obvious further source of ambiguity not eradicable by any degree of clarity and rigor in the subsequent deductive procedures.

So the first danger I warn against in the logicizing of psychology is that of 'pseudo-clarity.' Symbolic logic may and should help to clarify, but it offers no automatic device for exposing all ambiguities. The second warning I hinted at above may be called the danger of 'a too costly clarity.' There are diminishing returns in the process of eliminating ambiguities, and it is quite possible that one may pay too high a price in other values for eliminating some relatively unimportant ambiguity. Let me specify one such value, that of comprehension. It is possible to become too clear for comprehension. When a system becomes too complicated, when it requires too great a memory of definitions and specially invented, non-conventional symbols, its comprehension may require too much time and energy for the increased clarity it promises. This seems to me to be the case with *Mathematico-deductive theory of rote learning*, by Clark Hull and collaborators. Here about the simplest psychological phenomenon (rote learning of non-sense syllables) is considered. The

[12] To be accurate, I should say 'always.' But we may divide the undescribed but necessary properties into two classes: those that would be read into the description by any (or any trained) reader, and those that would not (and hence are a source of ambiguity). An instance of the former would be the fact that the experimenter gives his subjects instructions in a language they understand. An instance of the latter might be that the apparatus or techniques used furnish subjects unconsidered perceptual clues (as is frequently urged against experiments purporting to establish extra-sensory perception).

formulation of theorems and proofs in non-linguistic symbols is dispensed with (a sample proof is given in Appendix B). Yet the resultant 300 pages comes pretty close, I contend, to paying too high a price, in comprehensibility, for the clarity attained.[13] One way of attempting to dodge payment of this high price of clarity was, probably unconsciously, chosen by Hull in his 'miniature systems' (**19**, **20**), *viz.*, the use of a host of formally undefined, linguistic terms, with the trust in ordinary convention to give them meaning. Such a procedure, however, can hardly be said to be clearer than straightforward, careful exposition, though its form may give it an aura of clarity. Another method is by the use or development of symbolic conventions, so that the reader does not need to remember a host of items peculiar to the individual system. This, however, is only possible in a science where a high degree of theoretical unity has already been attained.

Another value supposedly arising from the logicizing of psychology is an increase in agreement between psychologists, an elimination of useless controversy, and a greater stability in basic concepts (**41**). This is to grow immediately out of the first value enumerated above (greater clarity), on the supposition that at least much disagreement in theoretical psychology is purely verbal.

This optimistic assumption would seem to be quite plausible. But there are two rather important considerations to be urged against it. First, there is the actual fact that various psychologists adopting symbolic technique, in some form, disagree on basic concepts, and apparently quite in conformity with their pre-logistic positions. That is to say, the use of symbolic logic, to date, does not seem to have appreciably increased agreement amongst psychologists (except the agreement to use it—which is not the value urged in its favor). Consider a few examples. Johnson (**23**), by developing for the concept of causation symbolic analogues of Whitehead and Russell's material implication and Lewis's strict implication,

[13] Of course this is a relative matter. One is willing to pay a high price for a symbolic accuracy such that it furnishes a real ground for testing important, rival theories of very general scope, *e.g.*, the general theory of relativity as against Newton's gravitational system.

argues for allowing telic and mnemic causation to be retained as explanatory principles along with efficient causation. Hull (**20**) attempts to reduce telic to efficient causation, by postlational technique.[14] Again, Hull (**20**) claims that his miniature logical system indicates that consciousness has no logical priority in the explanation of adaptive and moral human behavior. Miller (**32**), however, though an advocate of symbolic technique in psychological theory, claims that Hull does not make this out.[15] Furthermore, Pratt (**34**), an advocate of logical empiricism and operationism, claims that the primary data of psychology are neither physical nor mental, since no significant distinction can be made between physical behavior and consciousness. Likewise, as Bills (**3**) points out, both Kantor (**25**) and Stevens (**42**) are operationists, yet Kantor would restrict psychology to the study of inter-organic behavior (behaviorism), whereas Stevens would allow it to include studies of sensory attributes (as in classical, introspective psychology).[16] Lewin (**29**) uses a psychological interpretation of the mathematical discipline of topology to establish an obviously gestalt approach to behavior, whereas Hull (**19**, **20**) uses a postulational technique to establish an associationistic (Pavlovian) account.[17] I do not claim a comprehensive acquaintance with the psychological literature. However, it may be of some significance to report that I have yet to make my first discovery of the elimination of a basic theoretical disagreement between psychologists through the adoption of techniques of symbolic logic.

The second consideration weighing against the optimistic assumption that the use of symbolic logic will increase agreement amongst psychologists (beyond the agreement to use symbolic logic) turns on the nature of symbolic logic itself. In the first place, there is no necessity for agreement in the type of system used. Lewin (**29**) starts with a mathematical

[14] I am not acquainted with Johnson's views. But Hull in (**20**) has simply recast ideas previously stated in the form of ordinary exposition (**15**, **16**, **17**, **18**).

[15] I have at least Kantor (**26**) as authority for stating that this is in line with Miller's own pre-logistic acceptance of consciousness as a basic explanatory concept. Hull's behaviorism certainly antedates his espousal of symbolic logic.

[16] This is clearly in line with their non-logistic positions. See (**24**, **43**).

[17] This likewise is clearly in accord with their pre-logistic writings.

system already developed and proceeds by giving it his type of psychological interpretation. Hull (22) develops his own system, with his own postulates and undefined concepts. Logically, there are innumerable systems, any of which is as good as any other, if it satisfies certain logical requirements.[18] Further, for every formal system there are indefinitely many interpretations giving it psychological significance. It is thus easy to see that almost any psychological theory (not riddled with contradictions) can be formulated, without modification beyond recognition, as an interpretation of a logical system. And if the burden of agreement-gaining is put upon operational definitions of formally undefined terms in such systems, the case for optimism is not much improved. For, practically every important psychological theory can formulate its basic concepts operationally. Introspection, extrospection, Gestalt, association, consciousness, overt behavior—all are capable of operational definition (in the same sense in which any one is). The truth is, logic is exceedingly tolerant; it excludes only the impossible (self-contradictory). I cannot see how it will help much in bringing agreement into psychology.[19]

The danger I here warn against is naïve optimism in the agreement-producing power of the use of symbolic logic.[20] The danger, of course, lies in the possibility that after a great expenditure of effort in the logicizing of psychology it should be discovered that agreement is not thereby appreciably furthered, and philosophy, and symbolic logic in particular, be cast aside with as much disgust as it is now sought with avidity.

Furthermore, it is claimed that the logicizing of psychology will be valuable in increasing deductive rigor, namely, in increasing the importance of theoretical development as contrasted with sheer accumulation of observational data (27).

[18] See Katsoff (27).

[19] I might add that it seems to me that certain types of basic theoretical disagreement are very salutary in any science. An automatic or purely methodological device for insuring agreement should be viewed with great suspicion.

[20] A suggestive parallel from philosophy is the basic metaphysical difference between the authors of *Principia Mathematica*, Russell developing an atomism (38, 36, 37), Whitehead an organicism (46). It might of course be said that they did not use their symbolic techniques in constructing and testing their metaphysics.

Here again we have, I think, an excellent aim, but one accompanied by real dangers, of which I shall mention two—the danger of 'lulled criticism' and the danger of 'a too costly rigor.' When the mere form or appearance of deductive rigor is sufficient (as is often the case) to discourage criticism, the actual presence of complete rigor is even more sedative. That is to say, there is normally a generalization of attitude from "I can't criticize the deductive rigor of this system" to "I can't criticize this system." But even the most rigorous logical system, when given a scientific interpretation, can and should be seriously challenged. Its primitive elements should be challenged, especially by ascertaining whether conclusions derivable from them are in accord with observational data. This is quite generally recognized. But another challenge, scarcely if ever made, should be directed toward the very principles of deduction. What deductive procedures does the system use, and what restrictions as to scientific interpretation of the system do these require? If, as I shall contend, all important logical calculi to date are extensional, in their deductive principles, this would involve the omission of any intensional differences not revealed by extensional differences. Just what price would psychological theory have to pay to adhere to such a restriction?

Deductive rigor, like clarity, can be purchased at too high a price. Like clarity, it may be attained by too great a sacrifice of comprehensibility. I would here stress, however, another value with which it might conflict, the value of subject-matter. I would generalize the warning of Henry E. Garrett: "When working with mathematical symbols we should guard against the real danger that one may so forget his primary psychological interest that—like the grin of the Cheshire cat—only the mathematics remains" (**14**, p. 521). The danger, for psychology, is that psychological phenomena will be treated as merely illustrative of some logical system or method. This is clearly the case with the (biological) 'specimen' theory in Woodger (**48**); his real interest is in the 'metatheory.' I cannot avoid the feeling that this is also true of Hull (especially **22**). It is extremely valuable for logic to have

psychologists (and other scientists) aid in furnishing illustrative materials. But this is a contribution to the development of logic, not of psychology. It is important that psychologists continue to perform their own distinctive function.

Once more, we are told that the use of symbolic logic will greatly facilitate the excision of 'metaphysical entities' from psychological theory. I have found that 'metaphysical' is a cuss-word in current psychological literature.[21] But even cuss-words frequently have a core of rational meaning. So with 'metaphysical,' which, leaving out its function of emotional relief, seems pretty generally for psychologists to mean unobservable. 'Unobservable' is, however, not free from possible ambiguity. Hull (**22**, p. 3) classifies unobservables as "(1) those which, if conditions were as favorable as they might conceivably be, could really be observed, and (2) those which are inherently unobservable." He proceeds (p. 4) to identify this second class with 'purely symbolic constructs.' I am inclined to think that neither Hull nor the majority of psychologists would dub either of these (*viz.*, entities which physically, but not as yet technically,[22] can be observed, and merely symbolic constructs), 'metaphysical entities.' So I suggest as a stricter rendition of the rational meaning in the psychologist's use of this cuss-word: an entity is 'metaphysical' if it is existent (not merely a symbolic construct), and if it is inherently unobservable (its observation would conflict with known laws of nature).[23]

Now it is clearly foolish and unscientific to deny that there are metaphysical entities in this sense. Advocates of symbolic logic generally do not attempt it. They simply say that all reference to or explanatory use of metaphysical entities

[21] I hope to rectify this unfortunate emotionalism. Will my readers kindly be on the watch for an essay in descriptive metaphysics I hope to publish soon?

[22] I use a distinction of Reichenbach's (**35**, pp. 38 ff.).

[23] It is difficult to make this last qualification absolute, since no absolute limit can be assigned to the development of new means of observation. *E.g.*, the electron-microscope opens up untold new regions previously considered forever closed to all observation. This should furnish a warning against the overzealous exclusion of metaphysical entities from scientific theory on the ground of their inherent unobservability. A metaphysical entity of one age may become an observable entity in another.

should be eliminated from psychological theory. Terms that are not operationally, or at most observationally, definable are to be treated as merely symbolic constructs, serving to facilitate deduction.[24] This, of course, is simply the program of logical positivism applied to psychology.

It is undeniable that theoretical psychology (particularly the highly speculative psychology of many philosophers) has grievously sinned in the past by a too ready 'explanation' in terms of unobservables, such as 'will,' 'reason,' 'the id,' 'mneme.' But I believe there is a serious danger in denying all use of metaphysical entities in psychological theory, namely, the danger of 'theoretical asceticism.' Judging by the analogy of other sciences, I believe psychology would lose much by this self-denial. The geneticist's genes (functioning *as* genes, *i.e.*, as determinants of hereditary traits) [25] have been and probably still are metaphysical entities, as likewise the physicist's electrons and protons, the sociologist's social institutions. Ought the psychologist give up consciousness (other than one's own), nervous processes (in the unmutilated, behaving organism), although these are undoubtedly metaphysical entities? I would suggest the biblical test: "By their fruits ye shall know them." If the assumption of certain metaphysical entities is highly fruitful in theoretical consequences that can be checked observationally, then it is legitimate.

This brings us, however, squarely up against another claim: the logicizing of psychology will increase the fruitfulness of psychological theory (**22**, 310). A theory is fruitful to the extent to which it raises problems that can be solved (or a solution thereof can be significantly approximated) by appropriate observational data. A problem is a case of ignorance, but not all ignorance has the problematic form. A problem always has a theoretical context; it is ignorance of

[24] Hull (**22**, p. 306) admits that, in his most recent and ambitious attempt at logicizing psychology, he has not achieved this, but he claims it is an achievable desideratum.

[25] I am aware that chromomeres in the chromosomes of cells in the salivary gland of *Drosophila melanogaster* have been identified, through certain correlations, with the genes of genetic theory.

something relevant to the truth of some theory. That is why the mere accumulation of facts is not scientifically fruitful. The scientist seeks not simply more facts, but facts that will help explain other facts by aiding in the verification of some theory.

The logicizing of psychology is to make it more fruitful by placing greater stress on theory, and more especially by emphasizing the explanatory rôle of theory. Again, however, we are given pause by the disparity between claim and achievement. As Miller (**32**, p. 465) admits, "As a matter of record, the history of science indicates that important discoveries have only infrequently resulted from such careful procedures [as those of symbolic logic]." [26] But let us note more carefully what these logicizing psychologists mean by 'explanation.' Says Hull (**21**, p. 30), ". . . nothing will serve as scientific explanation except logical deductions from known postulates." Koch (**28**, p. 24) is more explicit. "By 'explanation' in an empirical system we shall mean the following: If $K$ is an empirical postulate set and $S$ is a sentence derivable from $K$, then $S$ will be said to be 'explained' by $K$ and $K$ will be said to constitute the explanation of $S$." But if we add to this the current view that derivation is tautological, it would seem difficult, in scientific explanation, to avoid what Hull calls the 'fallacy of renaming,' *i.e.*, a mere restatement in the explanation of the phenomenon supposedly explained.[27] If one had the total set of derivable consequences of a set of postulates, 'explanation of' would be a symmetrical relation, *i.e.*, the totality of sentences asserting phenomena explained would be identical in meaning with the sentences giving their explanation; it would be wholly arbitrary which is designated 'explanation of' and which 'explained by.' [28] This, I claim,

[26] Clearly Hull's miniature systems are not appreciably more fruitful than his earlier exposition of his theories. He claims fruitfulness for the theoretical development of *Mathematico-deductive theory of rote learning*. I am not in a position to pass judgment on this.

[27] I suspect an excellent case could be made out for the proposition that in the very article in which he names and explicitly attempts to avoid the 'fallacy of renaming' (**21**), Hull has not escaped it.

[28] Neither Hull (**20**), in asserting that consciousness has no logical priority or ultimacy, nor Miller (**32**), in denying that Hull has shown this, are aware that there is

does not agree with general scientific usage. Furthermore, as symbolic logicians are well aware, for any given proposition there are an indefinitely large number of sets of primitive elements from which it can be derived. Probably the believer in explanation as meaning derivation would bring in Ockham's razor. He would say that though there are an indefinite number of explanations of any given phenomenon, those explanations are 'better' which explain a larger number of other phenomena as well. This assertion, however, supposes either that the 'goodness' of an explanation is wholly arbitrary on our part (a matter of economy of effort), or that good explanation is not merely logical derivation, but is a discovery of some causal uniformity in nature.

This brings us to the crux of the matter. Explanation, in science, is not merely a formal or symbolic matter—mere substitution of symbols explains nothing. In asserting that he has found an explanation for some phenomenon, the (logically unsophisticated) scientist does not mean to assert that he has simply discovered a logical process of derivation, a merely formal relation of symbols. He means he has found a causal process in nature. That was why Newton was bothered by gravitational attraction as an explanation. It involved instantaneous action at a distance. If gravity were simply a postulate, identical with the totality of gravitational phenomena, and explanation were the relation of deducing some of the latter from the former, Newton would not have felt any difficulty. But he supposed that to explain gravitational phenomena was to find their cause, yet that gravitational attraction was an extraordinary cause—not taking observable time to operate over long distances.

To explain, it seems to me, is to find a parallel in fact to some deductive system. Parallel to the tautology of the deductive system (resting on assumed identity of symbolic reference despite change of context and, often, of actual symbols used) there is, partly observed and partly assumed,

no such thing as purely logical priority. What is chosen to be prior in a given system (its primitive, as contrasted with its derived, elements), is a psychological matter: *e.g.*, any undefined concept can be defined by means of concepts defined in terms of it (these latter then becoming undefined).

causal law (resting on existential persistence—either simple persistence, *i.e.*, constance, or functional persistence, *i.e.*, invariance,—despite change of context). Thus all explanation involves existential assertion of persistence. The reason it seems plausible to take 'explanation' to be simply logical derivation is that symbols and their relations in a logically formulated scientific theory can be read in two quite different ways. They may be abstractly considered in their formal nature; they may also be taken as referring to objective fact.[29] Specifically, formal deduction requires only certain assumed, indefinite identities of symbolic reference; explanation replaces these by asserted existential persistences.[30] These persistences are of two sorts: unobservable or hypothetical persistences, and partially observed or empirical persistences. Examples of the former are electrons, social institutions. Many excellent examples of the latter are found in Sherrington (**40**). For example, he finds that conduction through synaptic nervous processes is irreversible (as contrasted with nerve-trunk conduction). This then serves as an explanation of the unidirectional character of reflex action ($S \rightarrow R$). But it is explanatory only if it persists. He found it the case only with isolated preparations; he assumes it the case with synaptic processes *in situ* in intact organisms.

If this analysis be correct, it indicates a further danger in the logicizing of psychology, namely, the danger of 'hidden assertion.' Explanatory entities (at least partially unobserved, existential persistences) are tacitly asserted, although it is supposed that the system contains no unobservables save symbolic constructs. This 'hidden assertion' occurs through an implicit existential interpretation of the system, translating

[29] Technically, we may be concerned with the 'logical syntax' or with the 'semantics' of the system. My contention is that in analyzing a scientific theory, 'derivation' is a syntactical concept, 'explanation' a semantical concept.

[30] For example, to find the value of $s$ where $t = 3$ and $g = 32$ from $s = \frac{1}{2}gt^2$. By formal mathematical procedure we get $s = 144$. This value may be said to be 'derived' from the expression, $s = \frac{1}{2}gt^2$ (ignoring many further assumptions strictly involved), but it is not 'explained' by it. However, the distance covered (144 feet) by a freely falling body at the earth's surface starting from rest and falling for three seconds *is* said to be 'explained' by the physical law, $s = \frac{1}{2}gt^2$; but only in that $s$ and $t$ are interpreted as properties of an identifiable, persistent body, and $g$ is an empirical constant, an observable uniformity.

symbolic identity into existential persistence. I think this is a real danger, as witness Hull's 'sense organ,' 'neural reverberation,' 'stimulus trace,' 'capacity to evoke a reaction' (**20**); and his 'stimulus trace,' 'excitatory potential,' 'inhibitory potential,' 'reaction threshold' (**22**).[31] Also witness Pratt's astounding attempt (**34**) to unite 'critical positivism,' which claims that concepts (such as 'physiological trace') are mere words whose meaning is reducible to direct observations ('protocols') and that all explanation is circular, with the doctrine that all significant psychological explanation is physiological,—in terms of what goes on inside our bodies.[32] It is a well-known psychological fact that attempts at complete denial of basic human urges produce subconscious satisfactions of those urges. So it seems to be with the psychologist's urge to explain.

Finally, the logicizing of psychology is advocated on the basis that it will eliminate or greatly decrease the rôle of immediate intuition, that is to say, of direct apprehension of unique, qualitative content, in psychology. This elimination is sometimes advocated on the score that apprehension of qualities is by necessity introspective, therefore private and unscientific. Or again, the current dogma is appealed to, that science can only deal with relations and correlations, therefore not with the nature of the items related.[33] Furthermore, science, it is supposed, is at least ideally wholly quantitative; hence it should deny itself any qualitative intuition.

Here we approach a very basic issue. I sympathize with the psychologist who feels that the introspectionist, phenomenological, qualitative approach has been tried long enough, and that its paucity of universally confirmable results augurs ill for any further development along this line. But I would urge that a too fervid eagerness to escape this scien-

[31] Bergmann and Spence (footnote, 2, p. 9) would deny any physiological reference to such terms of Hull: they are 'empirical constructs.' My 'psychological' reaction is: Why did he choose terms which in ordinary usage very clearly connote the supposed physiological causes of the observed phenomena used in their explicit definition?

[32] We should not be amazed at the result: two physiologies—a formal, psychological physiology, and a material, physiological physiology (**34**, p. 143).

[33] *E.g.*, in Pratt (**34**, p. 128), where, however, it seems out of harmony with his contention that all differences for science must go back to original differences of direct experiences, of 'protocols.'

tifically barren subjectivism leads into a serious and all-too-prevalent fallacy,—the fallacy of 'obliteration of intensional differences.' The popular trend in psychology, as in symbolic logic, is toward an extensional reading of terms and propositions, and though this has certain obvious advantages (in the direction of a mechanized, as contrasted with an analytical, methodology), it also is seriously one-sided. In dealing here with such a basic and pervasive tendency I must necessarily be over-simple and dogmatic.

Let me first give some definitions. The intension of a term is the set of properties which must be exemplified by anything if that thing can properly (by symbolic convention) be referred to by that term. The intension of 'triangle' is plane, three-sided, rectilinear figure. The extension of a term is the total set of things exemplifying the intension. The extension of 'triangle' is the class of all plane, three-sided, rectilinear figures. It is clear, on these definitions, that every term with extension must have an intension,[34] that the extension presupposes the intension. How does it happen, then, that interest in extensional relations can result in disregard of intensional differences? First, because attention is normally directed toward extension. This is due to the fact that what a term refers to is its extension; its intension is its tool, its means, not its object, of reference. Second, because certain relations highly useful in logical, mathematical, and correlational procedures are such that attention to intensional differences is normally decreased by their use. For example, there is extensional identity. Two terms are identical in extension if they refer to exactly the same objects. Two terms with the same intension always have the same extension, but the converse does not hold: terms of different intension may have the same extension.[35] Thus a logic which treats terms with identical extension as equivalent, as substitutable one for the other, is more powerful (applies to more cases) than one which

[34] All terms have intension. By convention, a term with no extension is said to have a 'null' extension. Even Russell, with his proclivities towards nominalism, admits (39, p. 12) that the intension of a term is logically prior to its extension.

[35] A traditional example is 'featherless biped' and 'rational animal,' each having the same extension—the class of human beings.

insists on intensional identity, and its use tends to discourage questions as to intensional identity or difference. Again, both the relations of genus-species and of variable-value are cases of partial extensional identity, and logics based on them are concerned primarily with extension.[36] Hence again, in the interest of a more powerful logic, it is natural that questions of intensional qualification be disregarded in attending simply to partial extensional identity. Thus we find that logical calculi, based as they are on allowable substitutions of terms, tend to disregard intensional relations (save those of terms directly referring to extensional relations).[37]

As a matter of fact, this tendency might be said to form a cardinal plank in the reform program of the logical positivists. They in effect tell us that terms having no extensional difference of meaning can have no difference of meaning whatever. This is involved in the verifiability theory of meaning, which implies that if two terms have exactly the same perceptual referents they have no difference of meaning. It is also required for the legitimatizing of a 'physicalistic' (behavioristic) translation of introspective language, such that, *e.g.*, 'stomach contractions' can be substituted for 'hunger pain,' 'excessive adrenalin secretion, etc.' for 'rage.'[38] In fact, I believe it is not an overstatement to say that the logical positivists virtually deny all intensional meaning, and, after the fashion of the most extreme nominalists, reduce meaning to extension. This

[36] It is true that any specific species must be designated through a specific intension (the differentia), and any particular value must be intensionally distinguished from others. But for many purposes of symbolic substitution (of a species for a genus as subject of a universal proposition, of a genus for a species as predicate of an affirmative proposition, of a value for a variable in every occurrence of the variable in a total expression, etc.) it is not necessary to specify the species or to particularize the value: any species of that genus, any value of that variable will do.

[37] Lewis tends to treat the extensional character of actual logical systems partly as an historical accident, partly as due to the more complex character of an intensional logic (30, pp. 35, 322; 31, pp. 89, 119). What I have said amounts to a different formulation of the latter; I do not mean to deny the former.

What I have asserted concerning the extensional treatment of terms can be extended, by proper reformulation, to the extensional treatment of propositions.

[38] It is perhaps not usually pointed out that this extensionalism would likewise (or rather, on much better grounds) deny any difference of meaning to 'figure' and 'ground,' to 'right' and 'left' sides of a visual field, and in short to any perceptual correlates such that any experience of either is also an experience of the other.

seems the purpose of Reichenbach's 'zoological dictionary.' He would give meaning to the terms in 'This steamer has two funnels' by putting steamers, funnels, the variables $x$ and $y$, and the relation of having into cages marked with the appropriate names (**35**, pp. 35, *passim*). Also, it seems to be at the basis of Reichenbach's treatment of similarity, whereby a statement of similarity can be reduced to a disjunctive statement concerning the things said to be similar (**35**, pp. 171 ff). Likewise it seems involved in his contention that any abstract proposition is reducible to a set of concrete propositions (**35**, pp. 95 f.).

This tendency toward disregard of intensional differences finds a congenial reception in psychology, which, in its own way, was developing in the same direction. I refer to what may be called 'correlationism,' of which operationism is one form. For the sake of simplicity, let us consider perfect, 1–1 correlation: wherever there is an exemplification of a certain property, *A*, there is the exemplification of another property, *B*, and *vice versa*. *E.g.*, it might be found that having the highest I. Q. in one's school-class is in 1–1 correlation with having the highest average grade (and so on, for lower rankings). This is clearly a case of identity of extension along with diversity of intension. One reason why there is a natural tendency to disregard the intensional differences of terms in 1–1 correlation is that the psychologist is interested in establishing the correlation. This interest may be the practical one of using either correlate as evidence of the other. Or it may be theoretical, as indicative of a direct causal relation between, or a common cause of, the exemplifications of the correlated properties. But the fact is that psychologists have been greatly interested in finding correlations, little interested in retaining the (intensional) differences of the correlates. Add to this, ease of theoretical calculation and the power of denying facts that disagree with one's theory, and the result is indulgence in the 'fallacy of correlational definition.' I mean by 'correlational definition' definition of a term by a correlate, with the consequent obliteration of an intensional difference. *E.g.*, a correlational definition of 'blue' would be: the retinal

stimulation of a normal organism by light waves between 4250 Å and 4750 Å in length, or the definition of memory as the excitation of a previously conditioned $S \rightarrow R$ pattern.

The use of correlational definition with *malice prepense* (that is to say, to obliterate facts embarrassing to one's theory), though distressingly frequent is logically extremely weak. The general form of the practice is as follows. Exemplifications of $A$ are theoretically embarrassing facts; exemplifications of $B$ are quite proper. Hence when it can be claimed that $A$ is in 1–1 correlation with $B$ there is great relief: $A$ is defined as nothing but $B$. Thus, *e.g.*, when Hull wishes to eliminate the conscious phenomenon of anticipation, he defines it in terms of what he takes to be a correlate of it: "An organism is said to *anticipate* a state of affairs when there is active throughout the behavior sequence leading to the state of affairs a fractional component ($r_G$) of the action associated with the state of affairs" (**20**, p. 16).[39]

Some of the weaknesses in this procedure are: A 1–1 correlation can be established only if the terms have intensional difference.[40] Thus the attempt to eliminate one of these intensions by reducing it to the other involves one in self-contradiction. If it has been found that 'anticipation' and 'fractional component of a goal reaction' are in 1–1 correlation, the former cannot be identified, in intension, with the latter. *I.e.*, if it has been found that every situation in which anticipation occurs is *also* one in which $r_G$ occurs, it follows that anticipation is *not* $r_G$. Thus strictly neither of a pair of

[39] Hull might claim that he is not using 'anticipate' in its ordinary sense, but as a purely technical term. If so, he would doubtless say the same for his 'success,' 'failure,' 'desire,' etc. But why then did he choose these popular terms for his technical use, define them by what are obviously taken to be the behavioral correlates of their popular meanings, and pride himself upon the elimination of consciousess from his primitive elements?

[40] In the extreme, terms identical both in extension and intension are sometimes referred to as 'in 1–1 correlation,' e.g., 'father' and 'male parent.' It is in fact the suggestion that the difference between correlates is wholly verbal that gives extensional definition its plausibility. I would distinguish between 'definitional correlation' and 'correlational definition' as follows: definitional correlation correlates terms by giving them identical definitions (*e.g.*, 'father' and 'male parent'); correlational definition attempts to define a term as identical in meaning with another on the basis of an observed correlation in their referents (*e.g.*, 'hunger pains' defined as stomach contractions).

correlates is to be reduced, in intension, to the other. This can be put less strictly by saying that there are no grounds for preferring reduction in one direction to that in the other. Hull's definition of anticipation is not logically preferable to the following definition of $r_G$; "an organism is said to exhibit a fractional component of an action associated with a state of affairs ($r_G$) throughout a behavior sequence leading to that state of affairs whenever the organism anticipates that state of affairs." [41]

Again, correlational definition is logically restricted to terms whose extensions are denumerable classes. This is so because only denumerable classes can be put in 1–1 correlation.[42] In fact, the restriction is much greater if one wishes to avoid the 'inductive leap' in one's definitions. Consider the definition of blue as the retinal stimulation of a normal organism by light waves between 4250 Å and 4750 Å in length. If the extension of 'blue' and of 'retinal stimulation of a normal organism by light waves between 4250 Å and 4750 Å in length' is not a denumerable class, the 1–1 correlation on which this definition is based is theoretically impossible to establish. But in any case, the 1–1 correlation has not actually been established. On the basis of a very few instances a tremendous generalization has been constructed. It is rather dangerous to call such faith in unobserved fact a 'definition,' for calling it such tends to make one disregard the inductive leap involved, a negligence not corrected by dubbing it a 'convention.'

Finally, it should be (but apparently is not) obvious that

[41] *Cf.* (34, pp. 105–6). It might be said that Hull's definition of anticipation is not *logically* preferable to Hall's definition of $r_G$, but that it is *scientifically* preferable, since that which is scientifically unmanageable can be (by Hull's definition) replaced by that which can be quantitatively and experimentally determined. Now, it may well be true that $r_G$ is more scientifically manageable than anticipation. This might then warrant the scientific investigation of the former and not of the latter. But it does not warrant the reduction of the latter to the former, and especially not a reduction on the basis (which on this view could not be scientifically founded) of the 1–1 correlation of the two.

[42] It is sometimes said that an infinite class can be put in 1–1 correlation with proper parts of itself. This is acceptable only if one admits that 'infinite class,' 'proper part,' '1–1 correlation' are here read in intension. Whether there is any extension of these terms in this proposition (or whether their extension is 'null') is a moot question. Terms referring to extensional relations, like all terms, have intension.

the intuitive factor in knowledge cannot be eliminated by correlational techniques and definitions. It is impossible to determine the extensional correlations of properties unless one is acquainted with the properties *per se*. If one attempted correlational definitions of all one's terms, one's system would remain empty (without empirical reference) and either circular or involved in an infinite regress of definitions. An interesting suggestion of this latter possibility is found in Hull (**21**, p. 11). He defines 'stimulus equivalence' in terms of (as correlated with) response equivalence, and 'response equivalence' in terms of goal equivalence, which is identified with equivalence of reinforcement. Why he should stop here is logically unanswerable. If he does not like reliance upon intuition in the determination of equivalence of stimuli, why is he satisfied with it in the case of equivalence of reinforcement? [43]

Operationism, as I see it, is a form of correlationism not consistently carried through. It is based on a denial of the possibility of reliable intuitive knowledge of objects and objective properties, and requires that in place of such we must substitute (through correlational definition) a knowledge of our operations of determining these objective properties. Thus it falls into the general danger of correlationism, namely, that of obliteration of intensional differences. It may be that we never experience blue without going through an operation of discriminating it from other colors, as Stevens would contend, yet to ignore the difference of blue from our discrimination of blue is to obliterate an important intensional difference. As many writers have pointed out [44] operations by which we determine some objective property presuppose a non-operational acquaintance with the property. Einstein constructs an operational definition of simultaneity of distant events only on the basis of an intuitive acquaintance with the simultaneity of near events. Without this acquaintance, the locus of relevant application of the operation becomes wholly undetermined, and thereby the operation becomes wholly

[43] Actually, equivalence of reinforcement can be determined only by an equivalence of stimuli—viz., of certain stimuli acting upon the experimenter.

[44] *E.g.*, (**34**, pp. 103 ff.; **12**, **13**).

undetermined, also. Take the operation of determining length by the use of a meter-rod. Now suppose all direct experience of visual, tactile, and other qualitative contrasts in objects to be measured is eliminated. Obviously there would be no restrictions on where to start or stop the operation; there would be no length to be measured.[45] The emptiness of operations without intuitive acquaintance with the objective properties they determine seems strikingly apparent in Stevens' basic operation, discrimination (see especially **42**). He would seem to reduce all operations to the one undiscriminating, empty operation of discriminating. How the discrimination of blue can be discriminated from the discrimination of red, apart from the blue and red discriminated, I do not see, but if they are discriminated through the blue and red, then we have a non-operational, an intuitive, basis of operations.

Operationism, however, does not carry correlationism consistently through. Correlational definitions of objective properties are fine, but when it comes to operations, good correlations are not apparently a sufficient ground for identification; intensional differences are to be retained. At least for Bridgman, both in his earliest (**6**) and in his latest (**7**) formulation of his 'operationalism,' he insists that the basic value of operational definitions is their warning that we are not to forget differences in procedures even when they yield the same results. Thus, optical and tactile determinations of length may never disagree (beyond probable error), yet we ought to designate them by different terms, say $\text{length}_1$ and $\text{length}_2$. It is obviously this insistence upon retaining intensional differences of operations which has led to the widespread criticism (summarized in **44**) that operationism logically ends in an unmanageable multiplicity of concepts. Stevens (**44**) attempts to meet this objection by carrying correlationism a step farther. When there is sufficient ground for believing in a 1–1 correlation of two operations, then they can properly be identified with one another; *e.g.*, length as determined by a

[45] Of course the meter-rod itself would also vanish from our experience, and hence any operation of using it.

theodolite and by a meter-rod. This procedure, I fear, would lose the chief value of operationism in the eyes of its founder (Bridgman). In any case, it would not eliminate the intuitive factor. To ascertain that two operations are correlated requires an intuitive acquaintance with both operations. To find a correlation between the use of a theodolite and the use of a meter-rod presupposes direct acquaintance with these instruments in operation.

The danger in operationism of obliterating intensional differences is not hypothetical, but very real. It is found in all attempts to disparage phenomenological investigations, so far as grounded on the contention that there are no differences, or no differences of importance to psychology, save those of psychological method (or even, in extreme cases, of behavioristic method). It seems so scientifically sophisticated to reduce sensory differences, for example, to differences, stimulational and reportorial, in the techniques of 'determining' these sensory differences, that the obvious qualitative uniqueness of the sensory differences is often ignored, if not denied.[46]

Let me summarize. In connection with each value claimed to follow from the logicizing of psychology, I have indicated a danger or dangers. In connection with the value of increased clarity, there are the dangers of pseudo-clarity and a too costly clarity. Joined with the value of greater agreement is the danger of naïve optimism. Associated with the value of enhanced deductive rigor are the dangers of lulled criticism and a too costly rigor. Growing out of the value of excision of metaphysical entities from psychological theory is the danger of theoretical asceticism. Along with the value of augmented explanatory fruitfulness are the fallacies of renaming and of hidden assertion. Finally, the elimination or decrease of the

[46] It might be held that psychology is not to deny the existence of qualitative content in experience, but only to deny itself the right to investigate such content, and that operationism is the method of doing so. Far be it from me, a philosopher, to tell psychologists what should or should not fall within the scope of their science. But it does seem that such self-denial is rather extreme, and if this observational asceticism be combined with theoretical asceticism, psychology would seem to be in imminent danger of denying itself entirely out of existence.

rôle of qualitative intuition, as promised by correlationism and operationism, is accompanied by the danger of obliteration of intensional differences.[47] However, though these are real and serious dangers, they are not inescapable. The present paper is not a condemnation of the logicizing of psychology; rather, it is a warning—a warning that a promising novel tendency in psychology may easily degenerate into an ephemeral fad if its adherents do not recognize the limits of its usefulness.

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[47] I have not mentioned, since it has seemed to me rather too naïve, a value often actually urged as accruing from the logicizing of psychology, *viz.*, that psychology will thereby more closely resemble physics. Imitation of one's elders is a sign of immaturity. If psychology cannot justify the use of symbolic procedures by showing their relevance to *its own* subject matter, it had better not attempt *any* justification of them.

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[MS. received August 12, 1941]

# HOW SHALL WE STUDY INDIVIDUAL DIFFERENCES?

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## I. Introduction

The last fifteen or twenty years have seen a great increase of interest in individual differences. Indeed, a new division of psychology, differential psychology, has come into being whose province it is to study the differences between individuals and between groups.

The approach of the differential psychologist is objective and quantitative. "Individual differences are quantitative rather than qualitative" is the way the author of a widely used text in differential psychology puts it (**2**, p. 29). The authors of another text state: "While perhaps there are some differences between persons which we are obliged to recognize as qualitative, the vast majority of differences with which we shall be concerned may be called differences in amount and are accurately described as quantitative differences" (**5**, p. 58).

The field of individual differences is not, however, exclusively the domain of the differential psychologist. Clinical psychologists and students of personality, for example, are also engaged in the study of individual differences, whether or not they call it by that name. They by no means restrict themselves to quantitative methods, though they use test results and the like in their case studies and descriptions of personality. The experimental psychologist, too, is interested in individual differences (though more often than not they are incidental to his major purposes of investigation); he often describes variations among individuals by means of objective, observational or introspective methods.

The writer maintains that, notwithstanding their great services to psychology, quantitative approaches do not give

an accurate picture of individual differences. To provide an accurate description of differences between individuals he believes psychologists must frequently depart from their testing and statistical methods. He will seek to show that non-quantitative techniques, including introspection, may play an essential part in describing and understanding individual differences.[1]

In what respects, specifically, do the quantitative methods of the differential psychologist fail to give an accurate description of individual differences? First, quantitative treatment, *per se*, does not describe the methods of work used by a subject as he performs a task; second, it does not depict adequately the pattern of behavioral processes involved; thirdly, preoccupation with quantitative method causes one to lose sight of important aspects of individual personalities and therefore of differences between personalities. Let us examine each of these statements in more detail.

## II. Methods of Work

Many psychologists have investigated methods of work, but the implications of their studies for differential psychology have not been stressed. This point was made very forcibly by R. H. Seashore (**15**) who insists that methods of work may sometimes be as important as are biological and training factors in accounting for variations between individuals. In fact, he continues, methods of work play a part in determining individual differences in all activities—sensory, affective and intellectual as well as motor. Human capacity cannot be predicted unless the mode of operation is specified and controlled. "Our studies of individual differences," concludes Seashore, "have all too frequently been carried out with inadequate qualitative analysis" (**15**, pp. 123–124).

The writer agrees heartily with Seashore. Consider for a moment some of the studies of methods which show the

[1] The term 'non-quantitative' is used rather than 'qualitative' to avoid etymological and philosophical dispute about the meaning of the terms 'quantitative' and 'qualitative.' By 'non-quantitative' the writer simply means techniques other than the usual testing and statistical methods.

dependence of performance on the kind of procedure used. Perhaps the best example is furnished by the many time and motion studies made in the industrial field. The investigations of Warden (19) and Husband (9) revealed the superiority of verbal over visual and motor methods in human maze learning. Bryan and Harter (3) showed that a change in method, such as from letter-habit to word-habit, may cause a rise in the learning curve for telegraphy. Ruger (13) and later Maier (10) found that success in solving problems of thought is correlated with the kind of procedure followed by a subject. In his experiment upon the effects of caffein on efficiency Hollingworth (8) long ago pointed out that individual differences in the cancellation test and in the character of the caffein effect upon performance seemed to depend largely upon individual ways of performing the task. Some subjects showed simple recognition, direct perception and motor response; in other cases a real act of discrimination appeared necessary for cancellation of a certain letter. In a study of thinking processes made by the writer (14), which utilized the disarranged words test, two contrasting methods were discovered—a 'whole approach,' in which subjects gaze at the pattern of letters as a whole without attempting to analyze or recombine them, and a 'part approach,' which consists of manipulating letters and combinations of letters in an effort to hit upon a cue which will lead to solution. In all the above cases quantitative performance was affected by the type of method employed, and the methods were discovered by non-quantitative means—*i.e.*, by observation and introspection.

Now it would be possible, of course, to say that subject A's maze learning *performance* is better than B's, as shown by time and error scores, no matter what methods each used. But would it be correct to say that A's maze learning *ability* is superior to B's, if A has used the verbal and B the visual or motor method? Surely it is inaccurate to speak of differences in ability or capacity unless we are certain we are comparing our subjects in the same terms—that is to say, with respect to

relative effectiveness of performance in the same kind of behavior. And let us remember that most of our commonly used tests (excluding achievement and personality tests) are referred to as tests of 'aptitude,' 'ability' or even 'capacity.' One reason for the low validity and prognostic value of some tests may well be that we know so little about how they are actually performed.

Going still further, the writer would ask this question: Is it not conceivable that many if not all of the component tests used in intelligence and aptitude batteries may be performed by two or more methods? Teachers of mathematics know their pupils use a variety of methods—counting on fingers, visual imagery, verbalizing and the like—in such simple operations as adding and subtracting. In solving anagram problems which belong to a certain category such as animals or flowers some subjects try to recall the names of animals and flowers which might fit the letters, whereas others react primarily to the letters presented. Why then is it not likely that other tests—same and opposites, analogies, completion, disarranged sentences, geometrical problems, syllogisms—and pitch, time, intensity, art judgment and wiggly blocks—may be worked at and solved in many different ways? They may or may not be; we simply do not know because psychologists have not taken the trouble to find out.

If such studies were made many tests would probably turn out to be performed in approximately the same way by all subjects, in which case quantitative measures would be appropriate indicators. Where varying methods were found to be employed one possibility would be to give instruction in that method which is shown to be the best until subjects cease to show further improvement, at which point they should be quantitatively comparable in terms of ability. Other possibilities might be preferable. In any event it seems decidedly worthwhile, for both theoretical and practical reasons, to make careful observational and introspective studies of methods of work as a valuable check upon the accuracy of statistical results.

## III. Patterns of Behavioral Processes

The question of patterns of processes involved in performance is less obvious than the matter of methods of work. To illustrate what is meant by individual differences in patterns of behavioral processes as subjects work at a given task, the writer reverts to his above mentioned study of the disarranged word or anagram problem (14). Here an effort was made to discover the various ways of behaving in which subjects differed most clearly. The most significant modes of behavior (purposely not called 'factors,' 'abilities' or 'traits') were found to be:

1. Speed of immediate reorganization, which was hard to identify beyond its being the time subjects took to solve words without recourse to any other method than inspection of the whole group of letters.
2. Responsiveness to cues, which referred to differences in tendency to react to such cues as correct initials, endings, etc. which appeared in the given letters or were hit upon during trial and error manipulation of letters.
3. Limitation or control of hypotheses, which was a sort of restrictive set that prevented suggested solution words from departing very far from the letters provided.
4. Use of analysis or reasoning, identified as a conscious search for frequently used prefixes, suffixes and combinations of letters.
5. Versatility of hypotheses, which meant speed and fertility in trying out new combinations of letters.
6. Susceptibility to interference and perseveration, which referred to the persistence or recurrence of unwanted combinations of letters.

Subjects whose quantitative performance was good were generally superior in these six respects, but their patterns of variables of behavior were not necessarily similar. For example, of two subjects whose performance was alike in time and accuracy one showed superiority in responding to cues, in limiting hypotheses, in using analysis, and in freedom from interference and perseveration; the other subject was much

more active and versatile in setting up new hypotheses, which seemed to make up for his inferiority to the first subject in other respects.

It seems to the writer that non-quantitative data of this sort are of value to the differential psychologist. Indeed they are necessary to give meaning to his quantitative findings. Several statistical studies (**6, 12, 18**), for instance, have shown that a decreasing correlational relationship accompanies increasing disparity between levels of difficulty. But the quantitative data did not suggest why this is so. With the use of introspection, the writer was able to show that differing modes of behavior characterize performance at easy and difficult levels; in terms of mental processes involved, easy and difficult items of the material used tend to be different kinds of problems.

Or again, many quantitative studies (see **7, 21**) have dealt with effects of practice and training on individual differences, and a great and perplexing variety of correlational results has been obtained. Guesses have been made as to what occurs in the individual subjects, but so far as the writer is aware none of the investigators has sought to discover by direct experimental study just what these changes are. The phobia of the differential psychologist for non-statistical methods is indeed a well-developed one.

What about factor analysis? Does not this newly developed technique discover the nature of mental organization and the patterns of behavior variables? The answer, in good academic fashion, seems to be 'Yes and No.' Yes, factor analysis shows what abilities enter into performance when a number of tests are taken by a group of subjects. But no, factor analysis does not show what processes and procedures are involved in the mental activity of a given individual when he works at a particular test problem. As Thurstone (**17**) says, when a test has a saturation of two or more factors there is no way of knowing by factor analysis whether the several abilities enter into the test for every subject or whether some subjects use one ability and other subjects other abilities for the same performance. When tests are developed which are

saturated with only one factor or primary ability it will be possible to compare individuals with respect to that ability.

Factor analysis is not a substitute for the experimental study of the individual. Thurstone recognizes the need for both approaches: "If it is known that a task can be done by two different methods which call for different abilities, *A* and *B*, then the test should be revised, if possible, so as to make the task less ambiguous. . . . Individual testing has the advantage in that the examiner can ask the subject to explain how he solves the test problems. . . . The revision of tests is sometimes indicated by discussing with a number of subjects how they do the tasks" (16, p. 210). Wolfle states, in his recent review of factor analysis, that additional approaches, such as clinical or genetic methods, may be necessary to "provide an understanding of the basic causal variables in a field" (20, p. 43). In concluding the description of his method of factor analysis, Thurstone says he prefers "to head as soon as possible to direct forms of laboratory experiments in terms of which the primary factors may eventually be better understood" (16, p. 204). One could, of course, argue that the experimental study of individuals would better come before rather than after the factor analysis itself, but such argument might be as fruitless as that of the priority of the chicken or the egg. In any case it can be agreed that factor analysis and direct experimental study of individuals are supplementary and mutually helpful rather than antagonistic approaches.

## IV. Personality of the Individual Subjects

The third criticism of purely quantitative investigations of individual differences is that the use of exclusively statistical methods leads to a disregard for the personality of the individual subject. Several outstanding students of personality have written on this point within the last few years. G. W. Allport declares, in speaking of differential psychology, that "its interest centers in the function or attribute that is isolated for study and not in the man possessing these functions. The individual is only a means, not an end" (1, p. 8). He

adds that it is elementaristic and that it implies the sum-total of an individual's scores on isolated functions constitutes his individuality. "The peculiar patterning of attributes within the single person is not considered" (1, p. 9). H. A. Murray divides the psychologists of personality into 'peripheralists' and 'centralists.' The former are mostly academic men addicted to the methodology of science. "Being chiefly interested in what is measurable, they are forced to limit themselves to relatively unimportant fragments of personality or to the testing of specific skills. The aim is to get figures which may be worked statistically," rather than to understand, as the 'centralist' seeks to do, the individual "enmeshed in his environment; his ambitions, frustrations, apprehensions, rages, joys and miseries" (11, pp. 9–10).

L. K. Frank is also outspoken in his criticism of statistical approaches to personality. "The standardized test," he says, "does not tell very much about the individual, *as an individual*, but rather how nearly he approximates to a normal performance of culturally prescribed tasks. . . . By the use of an all-over total figure for an individual, it becomes possible to assign numerical evaluations to individuals in various categories. . . . Having assigned him to a rank order in a group or class according to the standardized test, the individual is disposed of and adequately explained" (4, p. 393). Frank believes that the projective methods of studying personality come closest to revealing individuality, to giving insight into the "individual's *private world* of meanings, significances, patterns and feelings" (4, p. 402).

The issues raised here are important, but so far the differential psychologist has shown remarkably little interest in methodological problems of personality study. Doubts do not assail him; his faith in tests and statistical treatment remains unshaken. He gazes with lack-luster eye, if not with actual distrust (in common, be it added, with many other psychologists), upon non-quantitative methods such as subjective reports, psychoanalysis and projective techniques, though he admits the usefulness of the case history for an understanding of the development of personality. In his

working hours he continues to devise new questionnaires and tests, and to undertake further factor analyses. Sometimes new techniques are introduced, such as the factor analysis of individuals instead of tests. His goal is to discover the most valid traits and dimensions of personality, to measure the individual with respect to each and to present the results in a compound psychograph.

But what of the criticisms of Allport, Murray and Frank? Can quantitative measures give a valid representation of personality, or can they not? The question is of immense practical and theoretical importance. It demands an answer buttressed with evidence of a very complete sort; so far this has not appeared. Unfortunately many differential psychologists seem to have the attitude that those who criticize quantitative methods are ipso facto unscientific and unworthy of serious attention.

Perhaps investigators of individual differences have concentrated so much upon the differences they have overlooked the individual! Constant work with test scores, percentiles, S.D.'s, critical ratios and correlation coefficients seems to have caused the differentialist to forget that he is after all dealing with individuals whose personalities are dynamic, unique and amazingly complex. Surely it is clear that if we wish to contrast two or more personalities we must understand each as an individual before we can make meaningful comparisons between them.

The differential psychologist should be interested in all approaches to individual differences. He is the only specialist who gives primary attention to this problem. The work of clinicians and experimentalists throws some light on individual and group variations, but the chief interests of these psychologists lie elsewhere. The burden of proof is on the mental tester to show that he would not profit from the leads furnished in case studies, in the observations of clinicians and laboratory workers and in the introspections of subjects.

Aside from theoretical considerations there is an intensely practical matter involved in all this. A strictly statistical approach may lead an investigator to overlook certain aspects

of the personalities of his subjects to the extent that the validity of his results is seriously affected. The control of variables like attention, set, motivation and attitude is a case in point; when these are neglected or carelessly standardized there is great likelihood of error. The work of one of the writer's colleagues [2] is of interest in this connection, as it suggests the rôle that such factors as negativism may play in the determination of Stanford-Binet scores. Quite considerable increases in I.Q. were revealed just a few weeks after young children first entered school. These increases appear to be brought about in part by the increasing rapport of most children with the tester and with the whole school environment. It is perfectly possible that studies of this sort may throw more light on knotty problems like constancy of the I.Q. than is done by whole volumes of statistical elaboration and interpretation.

## V. Conclusions

It is granted that methodology of personality study is a highly controversial matter. Many approaches are possible and plausible, as illustrated in the explorations of Murray (11). Time will doubtless show that some are fruitful and some not. Psychologists, psychiatrists, mental hygienists, psychiatric social workers and others are showing a growing interest in non-quantitative techniques, particularly projective methods, partly because of their dissatisfaction with personality questionnaires and rating scales and because of the difficulty of finding adequate objective tests. It is significant that in this new trend the differential psychologist is playing little or no part. There are individual exceptions, of course; one statistically oriented psychologist known to the writer became interested in the Rorschach test and before long the interest developed into almost rabid enthusiasm.

It would be encouraging if differentialists generally showed a more eclectic spirit and ranged farther afield to find out at first hand what other investigators of personality are doing or striving to do. Both groups would benefit. The differential psychologist would learn more about views which differ from

[2] G. McHugh, Barnard College, in a study not yet published.

his own—always a valuable if not altogether agreeable experience. On the other hand, his rigorous scientific training could be of great help in criticizing and curbing the excesses of some personality analysts whose zeal outweighs their judgment. But if the differential psychologist refuses to play any part in developing and molding newer approaches to personality and personality differences, he may at some future time find himself not only out of step but actually left behind in the psychological procession.

In this discussion the writer has not attempted to oppose quantitative methods as such; he has merely set forth what appear to him at the present moment to be the chief limitations of exclusively quantitative studies of individual differences. In a word, psychology needs completeness of description of individual differences; this completeness is best provided by a synthesis of differing techniques of investigation, not by one technique alone.

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[MS. received August 12, 1941]

# THE LANGUAGE OF PSYCHOLOGY AND OF EVERYDAY LIFE

BY RUDOLF EKSTEIN

It is obvious that if we were to use a primitive language such as the language of an Australian tribe, we should not be in a position to say all that we are, at the present time, able to express through the medium of modern English. Certain existing tribes use languages consisting of only a few dozen words of very simple logical construction. It is not difficult to comprehend that only a small part of our experience or the thoughts expressible in modern English can be thought or communicated in such a primitive language.

Different authors have tried to construct artificial 'primitive' languages to determine how much such languages permit us to express. Such an undertaking helps to clarify logical problems and thus improves our scientific language. Most of these authors have in mind the creation of a 'Unity of Scientific Language' (Wissenschaftliche Einheitssprache) in order to help bridge the gap which exists between the various branches of science. Another approach to the problem of the construction of a comprehensive scientific language may be the study of languages, such as the Eskimo, which are a different type from those of *our* orbit of language.

We may compare the language of a primitive tribe to that of a child. At first the child, too, posesses only a small vocabulary with limited range of meaning. The significance of a word is given through the (written and oral) rules of usage. The average child knows fewer rules than the average adult, and adds new rules of usage to his knowledge of speech very slowly. More complicated ways of expression are built upon the primitive language of his early years. To give an illustration: At each successive stage of development, the child adds a new layer of language habits to his range of speech, just

as the tree acquires a new layer of bark each year. Every layer of the child's speech habits is a part of the whole speech system just as every layer of bark is a part of the tree system. Sometimes it is very hard to distinguish between the different layers of bark, and even more complicated to see the different layers of speech habits because there are no lines of divisions, language layers like bark layers, very often being partly distorted or destroyed.

The child's language grows in power of expression just as the language as a whole had to undergo a long process of growing, broadening and deepening. The child's speech depends upon his psychological growth. He hurts himself at the table, but he holds back his tears because mother spanks the 'bad' table. The adult of our day no longer believes that a thunder god actually thunders, but he is still in the habit of saying "it thunders," even though he does not mean that 'something' thunders. He who today asks "What is the cause of thunder?" considers the question of "Who or what thunders?" a senseless combination of words. But the old habit of expressing phenomena in primitive words, a habit caused by primitive theories of explanation, is still in use although the primitive theory has been abandoned. The words lose their old meaning and are adjusted to new usage. An examination of the different layers of language might throw some light upon its present problem.

In the English language as in other modern tongues there are many expressions (idioms) which bear out the fact that word material often assumes a meaning different from its former purely literal one.

In this essay, I should like only to call attention to some of those idioms which are used to express psychological facts, but which are derived from word-material used at first only to express physical facts (those related to space).

Will the following examples justify the conclusion that at first neither the primitive man nor the child communicates (thinks) psychological facts? Will they lead us to believe that the primitive man or the child already has a certain philosophy or theory, and therefore uses a language which is conditioned

by the theory? Or, in other words, is it true that certain limited experiences suggest language, and, that in describing new experiences we have to use expressions (idioms) of the previous language layers, thus enlarging, step by step, the old meaning to a more figurative one? Or, is, perhaps, a certain 'philosophical' attitude already the cause for the choice of speech habits?

The idioms in the following sentences are often used to express psychological facts: (1) Somebody is a *low* character. (2) He has a *high* cultural level. (3) Somebody is *high* minded. (4) He has *high* hopes. (5) Chin *up!* (6) He talks *behind* his back. (7) "My heart is *in* the highlands, my heart is not *here*." (8) "Should auld acquaintance be forgot and never *brought to* mind." (9) He *overcomes* his hardships. (10) It comes *into* his mind. (11) He *falls in* love. (12) He *gets back* or *lost* his memory.

Here, then, is word-material generally used for description related to space transferred to the psychological sphere. All the words (low, high, above, below, up, behind, bring to, come into, fall in, and so on) lose their original meaning and are used in a figurative sense. At first, the language probably served only the purpose of communicating (thinking) facts and events in space; therefore, later on man had to adjust his word-material to new experiences. Our language is very often inadequate in dealing with new problems. The process of adjustment is very hesitating and blundering. The language fumbles along to new experiences. But, this is the only way to progress and has its advantages as well as drawbacks. The richness, often even the obscurity itself, of our language helps to modify and widen its capacity for expression. The danger is, as we are trying to show, that the metaphoric usage of words leads to 'philosophical problems' which we cannot solve because of our misunderstanding of their true character. A great part of philosophical writing is due to the complete misunderstanding of these 'problems' and leads to metaphysics.[1]

[1] We accept metaphysics as an expression of an attitude, of a frame of mind, or a stimulant, as an expression of one's outlook or one's 'philosophy' just as we accept poetry or music. However, we do not think that metaphysics solves problems or

Some of the above-mentioned idioms and phrases of our language of everyday life may be used to show how they sometimes lead to 'philosophical problems.' Let us start with the idioms used in sentences 1–5: being a *low* character, *high* and *low*-spirited, having *high* and *low* culture, *high* hopes, and so forth. The terms 'high' and 'low,' originally used for descriptions related to space (for example: This is a high [low] mountain) are now used for descriptions of moral standards, cultural values, states of expectation, or of ideas. No word-material has absolute meaning but receives its meaning from the rules of usage. The rules governing a word however are flexible; in different types of sentences, words are used differently, thus changing their rules of usage and their significance. We shall use the term 'word-material' to denote any word whose meaning is as yet undetermined by rules of usage. The words in a dictionary constitute word-material. Conceptions are word-material which has received its meaning through rules of usage. Conceptions are words in sentences. The same word-material can be used for different conceptions. Usually we do not distinguish between words in a dictionary and words in sentences; therefore, we are likely to overlook the use of different conceptions even though the word-material might be the same.

The above-mentioned sentences (1–5) are examples of different sentence types. In each one, the same word material, 'high-low,' is used throughout but different conceptions are used as the word-material follows different rules of usage according to the different sentence types.

High and low mountain tops are compared by measuring their respective height. For example, we use units of the metric system to determine the height of a mountain. We know the methods of measuring distance, so giving the method of measurement, which is the method of verification of statements about the height of a mountain, we also give the sense

answers questions. Only in a metaphorical sense may we say that music, art, or metaphysics solves 'problems.' These 'problems' cannot be answered like questions Decisions in our life may be considered as answers to human problems. In this instance, the word-material 'answer, problem, question' is also used in a metaphorical sense.

of the statement about the low or high mountain top. The word-coupling 'high-low' receives its meaning from the sum of rules which determine the sense of the sentences in the above-mentioned sentence type.

But, high or low mountains are not high or low characters. A different scale of measurement is needed to compare high and low characters. Values of length and moral values have only the word-material 'value' in common. The word-material (value, measurement, high, low) in the latter example is used in a metaphorical sense only, and the comparison of the two systems of determining (1) high and low mountain tops, (2) high and low characters, shows that in the latter case the word-coupling high-low follows different rules of usage, or, in other words, has a different logical grammar. The fact that we usually do not speak of a *high* character but only of a *low* character is an indication of the different logical grammar of the word-material 'high-low' in both cases.

It is not easy to determine the value of a person's character because the set of rules differ with the nation, religious group, social class, political party, professional group, and even with the individual. One might say that this is also the case as far as the measuring of the height of mountains is concerned: one group may use the metric system, another group, the yard-stick; and a third group still another method. This comparison however is defective. Every sentence of the 'linear system' language can be translated into the 'metric system' language. The sense of the sentence: "The height of the mountain is 1000 yards," is the same as that of the sentence: "The height of the mountain is 914.4 meters." Different moral sets are not interchangeable because a moral standard signifies judgment, approbation, or disapproval of someone's attitude. We do not condemn a low mountain as we condemn a low or bad character. A moral judgment, the condemnation or praise of a person, depends upon one's outlook. We do not like certain human qualities; therefore, we consider them as undesirable or bad. Consequently, persons possessing these 'bad' qualities are thought of as low characters. Our personal wishes as to how people should believe and act determine our

system of moral values. A person using the metric system can translate every statement expressed in the linear system; therefore, we may say that we have an absolute system of length values in our rough physics of everyday life. Many philosophical writers on ethical problems are misled by the word-material (good, bad, low, high) to believe that there is also something like an absolute system of moral values. But, moral values are dependent upon personal decisions: they are related to different persons. Length values are interpersonal. The word-coupling 'high-low' follows different rules of usage when employed in the sense of 'moral values.' [2]

The usage of the word-coupling 'high-low' to mean 'high or low culture' differs from its usage when it signifies 'low character,' even though here, too, one's own outlook may decide as to what shall be called low or high culture. The expression is often used without any meaning of contempt or praise, and has only classifiable character to denote, for example, different stages of development of historical peoples.

The rules of usage of the word-material 'high-low' are somewhat different in sentences describing 'high or low spirits, high hopes.' These expressions are used in psychological descriptions of everyday life. For example, the ways of comparing a high-spirited person to a low-spirited person are very vague; therefore, we can give only a vague meaning to the word-material. Psychologists, 'measuring' high or low hopes, do not measure them in the same sense that we measure the height of mountains.

We see that the use of the word-material 'high-low' in areas other than in the field related to space enlarges the capacity for expression of our language. But there are also certain dangers when old word-material assumes new meaning. It leads to 'problems' of the type: "Are there any absolute moral values?" The true 'answer' to such a problem is not 'yes' or 'no' but consists in an analysis of the meaning, of the logical grammar of vague expressions. The question: "Do

[2] These remarks should not lead us to believe that psychological descriptions cannot be verified by different persons, but psychological descriptions are not moral judgments although they might lead to moral judgments. Descriptions need verification. Moral judgments are based on decisions.

*Exibotites* exist?" The name, a fiction of my own, can be answered only after we know what is meant by 'Exibotites.' The question is meaningless as long as we do not define Exibotites. The discontent of a person not knowing the answer to our question disappears after the person is able to determine the sense or senselessness of the question. A clarification of the term 'moral value' helps us to see that it is an offense against the rules of logics (not against facts) to link the term 'moral value' with the adjective 'absolute.' To know the field of rules is to know the meaning, and this is the answer to the philosophical problem.

Perhaps still more light can be thrown on the problem by considering other examples having implicitly the same difficulties. In examples, Nos. 5–12, we find again word-material, formerly used in descriptions related to space, now used in the field of psychological description. The new figurative and metaphoric use of the word-material that loses its old meaning and now follows different rules of usage is not recognized and might induce us to 'locate' psychic functions or events just as we locate physical things or events. (Compare the two sentences: He lost his book but recovered it later. He lost his memory but recovered it later. "Where was the book?" is a logical, admissible question. But does it mean to ask: "Where was the memory?") Many philosophical writers do not see the grammatical problem of our language, and instead of trying to analyze complicated language patterns, they 'examine' the two realms of the soul and of the body. Statements of the kind "The soul is not related to space," are not descriptions of the soul, but only illustrate the fact that word-material formerly related to 'physical' descriptions cannot be used in the same sense in the psychological area.[3] We speak of a current of life, of a current of time, or an electric current. But, the word-material 'current,' formerly used to describe moving water masses, undergoes many changes of rules of usage in the above-mentioned examples. If we neglect the logical consequences of these changes, we then may feel bound

[3] It is, no doubt, clear by now that the term 'psychological *area*' is also meant in a metaphorical sense.

to ask for the secret of time or life, or for the true nature of electricity. 'Questions' of this kind have no scientific meaning although they may be an impulse for scientific research and may lead to clear and to real questions. The philosopher 'answers' them in finding the new rules for word-material which is now used in a different logical structure to describe new experiences or to express new theories. We may define the task of philosophy as 'to ask clear questions' which experimental and empirical science must answer.

It would be a mistake to believe that only the language of every-day life uses such metaphors leading to obscurity and to metaphysics. Terms like 'electric current,' 'power,' or 'wave,' as used in physics, show that even the exact sciences make use of the adaptibility of the language. These sciences use word-material the meaning of which is now determined by other rules only partly covering the rules of the original conception. The science of psychology as well, undeveloped in comparison with the so-called exact sciences, again and again uses terms derived from word-material previously related to space. Psychoanalysis, for example, uses terms like *un*conscious, *subconscious*, *suppression*, *repression*, *replacement*, *superego*, etc., which lead one to believe that the make-up of personality is like a building with different rooms and stories. A picture of that kind may be helpful in explaining psychological facts, but is dangerous if we try to draw all logical consequences from such a comparison.

Experiences and interests are different with different individuals and also with different nations; therefore, we find that the development of language is different in different persons, interest groups, groups of scientists, or nations. Different experiences or interests lead us to use different means of expression, and develop our word-material differently. The differences, although they may seem very small in one language, greatly influence modes of expression and thought.[4] This is the case to an even greater degree with different

[4] For example: American and British English, English as used in different psychological schools, the English as used by different professional groups or different political parties or religious groups.

languages, as, for example, English and German.[5] English, German and French have about the same logical set-up; therefore, we are still able to 'translate' a work of philosophy from one language to the other. It would be more difficult to try to 'understand' Chinese philosophy. Philosophical thought is dependent upon the logical structure of a language. A language is an instrument that steadily improves and changes and adapts itself to new purposes. The constant readaptation of word-material changes the meaning and therefore the rules of the used material.[6] These rules of usage are often changing without our knowing all the logical consequences of the change; they therefore always cause new philosophical problems which are answered through clarification, through new determination of the rules.

A historical approach to language material, one which tries to discover how the rules change and develop through new adapted forms of usage helps to remove 'philosophical' problems (*Scheinprobleme*). This, then, is the task of philosophy. Real problems are to be solved by science. Philosophy does not look for the truth but only for the sense of sentences and theory, and the meaning of conceptions.

[MS. received August 25, 1941]

[5] It seems to the writer that the idealistic philosophical systems of German philosophy could not have been created in the English language, that, on the other hand, the Philosophy of Pragmatism could have its root only in the English set-up.

[6] This readaptation is also partially accomplished by the addition of new word-material, new 'terms,' for example.

# REMINISCENCE IN THE ACQUISITION OF SKILL

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In connection with a survey of the literature on reminiscence (2), the writer found reason to believe that the phenomenon should be exhibited unambiguously in the acquisition of skills. Some typical investigations of the spacing of practice were examined with this in mind, and although the experimental conditions under which data were obtained were rarely entirely favorable for the appearance of reminiscence, additional computations showed rather clearly that it was present. This paper reports a reëxamination of several such studies.

The Tilton letter-substitution code was used in a study by Gentry (5). Several groups of *S*'s had 1-minute practice trials at the code, with 1-minute rest intervals interpolated after various amounts of massed practice. Bunch (1) has pointed out that giving more than one retention test to any group of *S*'s actually gives them more practice at their task, so that what may seem like reminiscence is only improvement through additional practice. Gentry's data are in a form which permits holding this factor constant by using groups which are equated initially on the basis of trial scores. One group then takes a 'retention test' (its next trial) immediately, while another is delayed for one minute. If only the *second* tests of the two groups are compared, practice effects are equal and the difference between their means is a legitimate measure of reminiscence during the rest interval in the group which has it.[1] This measure was applied at three different levels of learning in Gentry's data, as shown in Table I. The

[1] As several writers have pointed out, the use of averages may conceal or distort the amount of reminiscence shown in a given study. The proportion of *S*'s showing reminiscence might be a more desirable (or sensitive) measure, but individual data were not available in most of the studies mentioned in this paper.

zero-interval groups are those which had massed practice up to and including the trial shown in the fourth column, and then had the next trial immediately. The 1-minute interval groups are those which had massed practice up to and including the trial shown in the fourth column, then a 1-minute rest interval, then the next trial. These *last* trials, taken under the two conditions, are compared in each instance. The amount of reminiscence in the 1-minute rest interval is significant at the three levels of learning analyzed in this table, and there is some indication that it decreases as the initial level becomes higher.[2]

TABLE I

| Group | $N$ | Interval | Mean, trial 1 | S.E.$_m$ | Mean, trial 2 | S.E.$_m$ | $M_1-M_2$, trial 2 | S.E.$_d$ | C.R. |
|---|---|---|---|---|---|---|---|---|---|
| $M_8D_8M_{10}$ | 131 | 0 | 16.06 | .58 | 16.22 | .44 | | | |
| $D_8M_{18}$ | 141 | 1 min. | 16.08 | .45 | 19.15 | .35 | 2.93 | .56 | 5.23 |
| | | | Mean, trial 6 | | Mean, trial 7 | | $M_1-M_6$, trial 7 | | |
| $M_{10}D_8M_8$ | 120 | 0 | 20.73 | .42 | 20.80 | .43 | | | |
| $M_8D_8M_{10}$ | 131 | 1 min. | 20.86 | .39 | 24.12 | .50 | 3.32 | .66 | 5.03 |
| | | | Mean, trial 11 | | Mean, trial 12 | | $M_1-M_6$, trial 12 | | |
| $M_{20}$ | 88 | 0 | 20.35 | .54 | 21.05 | .55 | | | |
| $M_{10}D_8M_8$ | 120 | 1 min. | 20.42 | .50 | 23.65 | .52 | 2.60 | .75 | 3.47 |

Another study not concerned with reminiscence as such, but giving evidence of its existence, was reported by Snoddy (9). From various parts of his monograph on mirror-tracing the velocity scores [3] contained in Table II have been brought

[2] Lorge (7) also used the Tilton code, under conditions similar to those described above. Computations by the present writer show that a rest interval of 1 min. is more likely to produce reminiscence (based on group averages) than an interval of one hour or one day. However, Lorge's groups were small, and although there was consistent improvement in groups having rest intervals, it was not statistically reliable.

Gentry and Lorge also used a mirror-reading skill, under conditions similar to those described here; gains in rest intervals after the first trial are consistent, but superiority of rest groups over 0-interval groups is not statistically reliable. In addition, Lorge used a numbers-associates test which was very difficult. Further computations of these data were not made because of the sharp drop by the 0-interval group on its second trial.

[3] These scores were obtained by verbally pacing $S$ to produce a performance in which there was approximately one error per second during the circuit of a metal star pattern with notched edges. As $S$ improves, the velocity into which the time and error scores are transmuted becomes greater.

together. One group had two circuits of the star in immediate succession. Other groups had varying intervals between the two trials. Comparison of *second* trials again is a legitimate way of measuring reminiscence. However, S.E.'s are available only for the comparison of the 0-interval and 24-hr. groups. In this instance, there is unquestionable evidence of reminiscence, after only one trial. The slight inferiority of the rest-interval group on the first circuit becomes definite superiority on the second. Inspection of the second-trial means reveals that the amount of reminiscence is positively related to the

TABLE II

| Interval | *N* | Mean, trial 1 | S.E.$_m$ | Mean, trial 2 | S.E.$_m$ | $M_1 - M_2$, trial 2 | S.E.$_d$ | C.R. |
|---|---|---|---|---|---|---|---|---|
| 0 | 70 | 5.85 | .40 | 7.25 | .44 | | | |
| 1 day | 72 | 5.24 | .39 | 10.69 | .52 | 3.44 | .68 | 5.05 |
| 30 sec. | 80 | 5.76 | | 8.68 | | | | |
| 2 min. | 70 | 6.25 | | 10.00 | | | | |

TABLE III

| Interval | *N* | Mean, trial 1 | S.E.$_m$ | Mean, trial 2 | S.E.$_m$ | $M_1 - M_2$, trial 2 | S.E.$_d$ | C.R. |
|---|---|---|---|---|---|---|---|---|
| 0 | 19 | 162.68 | 15.20 | 150.42 | 14.15 | | | |
| 1 min. | 19 | 161.37 | 14.00 | 131.26 | 13.31 | 19.16 | 19.43 | .99 |
| 1 hr. | 21 | 162.52 | 17.91 | 111.57 | 6.81 | 38.85 | 15.70 | 2.47 |
| 1 day | 19 | 162.84 | 14.70 | 113.89 | 9.44 | 36.53 | 17.01 | 2.15 |

length of rest interval, with no drop even at the 24-hour interval.

Data similar to Snoddy's may be found in a monograph by Lorge (7). He, however, used scores which decreased in size as *S* improved; his *N*'s were small, so that differences between the no-rest and the rest groups are not significant. The trend is consistent and supports Snoddy's findings, as Table III shows.

Another motor-learning task which gives evidence of reminiscence is the pursuit-rotor. In a study by Doré and Hilgard (4), *S*'s were given three 1-minute trials with two 1-minute rest intervals interpolated. One group continued

with this procedure, taking its fourth trial after another 1-minute rest, while another group rested for 11 minutes. The data for fourth trials are compared in Table IV. Although the group having the longer rest interval was somewhat inferior on trial 3, it was definitely superior on trial 4. It should be noted that practice up to the crucial rest interval

TABLE IV

| Interval | $N$ | Mean, trial 3 | S.E.$_m$ | Mean, trial 4 | S.E.$_m$ | $M_{11}-M_{1'}$ trial 4 | S.E.$_d$ | C.R. |
|---|---|---|---|---|---|---|---|---|
| 1 min. | 25 | 160 | 17 | 197 | 16 | | | |
| 11 min. | 25 | 152 | 18 | 256 | 19 | 59 | 24.8 | 2.38 |

was spaced, which probably is not an optimum condition for the appearance of reminiscence. Also, there was no real zero interval.

This trend is supported by the trend in additional data (see Table V) taken from another paper by the same authors (3). The experimental procedure preceding the critical rest period was the same as in the experiment previously cited.

TABLE V

| Interval | Mean, trial 3 | S.E.$_m$ | Mean, trial 4 | S.E.$_m$ | Amount of gain | Per cent gain |
|---|---|---|---|---|---|---|
| 0 | 169 | 13 | 151 | | −18 | −11 |
| 1 min. | 170 | 17 | 220 | | 50 | 29 |
| 3 min. | 186 | 15 | 253 | | 67 | 36 |
| 11 min. | 182 | 14 | 316 | 16 | 134 | 74 |

Because certain S.E.'s are lacking, and particularly because the group having the zero interval shows a sharp drop on its fourth trial, statistical comparisons among the fourth-trial means are not made. The trend toward larger gains with the longer rest intervals is clear and consistent, although again it should be remembered that practice up to the crucial interval was spaced. It should be pointed out here, as the original investigators pointed out, that fatigue during a 1-minute practice trial on the pursuit-rotor may have something to do with the gain (recovery) in rest periods thereafter, so that

these particular pursuit-rotor data are not as clear-cut evidence for reminiscence as one might like. The use of shorter practice trials would probably avoid this complication. However, other pursuit rotor studies (**6**, p. 430; **10**, p. 186) and a pursuit-oscillator study (**11**, p. 193) support the trends of the Doré and Hilgard papers, even though the data they add are not in a form suitable for the present kind of re-analysis.

Because the pursuit-meter study of Renshaw and Schwarzbek (**8**) seemed also to support the Doré and Hilgard findings, some of their data were analyzed.[4] A cycle, or a trial, required *S* to follow with a stylus a large number of eccentric movements of a spot electrode. The measure of skill was the number of errors made in completing a cycle. In one group

TABLE VI

| Interval | *N* | Mean, cycle 1 | Mean, cycle 2 | $M_1-M_2$, cycle 2 | | Level |
|---|---|---|---|---|---|---|
| 0 | 7 | 238.0 | 213.1 | | | |
| 9 min. | 8 | 251.0 | 114.3 | 98.8 | 1.81 | 10% |

of *S*'s, the first practice cycle was followed by a 9-minute rest, then a second cycle. In line with the comparisons made previously in this paper this group may be compared with one in which the second cycle followed the first immediately. Unfortunately, as will be seen in Table VI, the groups were very small and not well equated on the first cycle. Nevertheless, since the no-rest group was superior on the first cycle, second cycles are compared; the resultant *t* is a conservative indication of the presence of reminiscence. Again there is a nearly reliable superiority of the rest-interval group.

The studies here cited serve as 'new' illustrations of the wide-spread occurrence of reminiscence, and at the same time illustrate the possibility that skill-acquisition is a suitable type of learning task in which to study the phenomenon. Because of the fact that much of the literature on the spacing of practice deals with skills, it seems desirable to study

[4] Professor Renshaw very kindly supplied the individual data upon which the computations reported here were based.

reminiscence in skills also. This will bring reminiscence into closer connection with our knowledge of the effects of the spacing of practice.

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[MS. received September 11, 1941]

# NOTE ON THE EXTERNALIZATION OF DRIVE

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In a recent article Anderson (1) has proposed a theory of externalization of drive from which he has deduced 29 predictions mainly concerned with maze learning by rats. So fruitful a concept in a field as basic as the role of motivation in learning deserves careful analysis. It is with this justification that I venture the following comment:

Anderson's deductions are based on an implicit assumption which, I believe, should be made explicit. Let me review his argument. Drive is defined as a neural mechanism leading to persistent behavior. A drive originally aroused by internal conditions, as in hunger, will, after repeated satisfaction in a constant external situation, be aroused by that situation or certain features of it. Thus in the course of learning a maze the hungry rat's drive becomes elicited by the food, the food box, and finally by the maze itself. This shift is held responsible for a number of phenomena, some observed, some predicted; *e.g.*, the disturbance of maze performance, late in learning, by removal of reward, change of reward box, or change to a different reward, and the improvement of performance by introduction of reward (latent learning) or by pre-feeding.

The implicit assumption underlying most of the predictions is that in becoming externalized a drive is not merely *aroused by* but also *directed toward* the reward or reward box. Unless this assumption is made, it is difficult to see how changes in the goal could affect performance in the maze before the goal was reached.[1] In other words, we must assume that an ex-

[1] In massed trials, it is true, the reward might exert a perseverative effect on the next trial, similar to that of pre-feeding. Muenzinger, Dove, and Bernstone (3) demonstrated such an effect by means of their 'endless maze.' There is no need, however, to restrict the present theory to massed learning.

ternalized drive is aroused, not merely by the goal object itself, but by some internal surrogate evoked before the goal is actually present.

Hull's system includes an ingenious mechanism, based on conditioning principles, whereby a sequence of responses may become goal-directed (2). Some readily available part of the goal response, by virtue of strong conditioning to the internal drive stimulus, occurs early in the sequence, where it may be conditioned to the corresponding external stimuli. This fractional anticipatory goal reaction, with its proprioceptive consequent, then enters into the successive stimulus complexes eliciting, *e.g.*, maze behavior. It may be thought of as a surrogate or symbol of the goal. Changes in the goal—introduction or removal of food, change of food or food box—will alter its symbol and thereby the responses conditioned to it. Externalization, in Hull's terminology, seems to involve three stages of conditioning: (1) conditioning of the consummatory or goal response to the internal drive stimulus; (2) conditioning of a fraction of the goal response to earlier external stimuli; (3) conditioning of overt responses to the proprioceptive component of the fractional anticipatory goal reaction. The spread of externalization to other parts of the situation, such as the maze itself, would be similarly described in terms of anticipatory subgoal reactions. It is worth noting that, aside from the internal drive *stimulus*, Hull's formulation omits mention of drive altogether. If we keep within his framework we have, I think, two alternatives. The first is to equate the 'neural mechanism leading to persistent behavior' with the fractional anticipatory goal reaction. The second is to follow Anderson, if I interpret him correctly, in conceiving of drive as a kind of middle term, to be roughly identified with 'tension,' between stimuli and responses in a behavior sequence. In this case we should say, concerning stage 3 above, that the *drive* was conditioned to the goal surrogate and the overt responses set off by the drive. There may be some advantage in this interpolation, although I fail to see it.

That some such mechanism is implied in his theory Anderson himself indicated when he said that the shifts in

motivation might be described in terms of Hull's conditionings or Tolman's hypotheses. His further elaboration of the theory, however, reveals the value of an explicit analysis such as the one here attempted. After externalization has occurred, he says, the drive may be 're-internalized,' *i.e.*, become aroused by a new set of inner, presumably cortical conditions. At this stage the rat, instead of just learning mazes, would 'go about looking for new mazes to learn.' The above analysis shows that this would not represent a new development but rather an extension of the old. The very process of externalization, in so far as it influences maze learning, involves re-internalization. The animal responds not merely to the food or food box but to some internalized representative of that food or food box. This is what we mean when we say he is 'looking for food.' Similarly if he is 'looking for a maze to learn' he must be responding to some inner symbol of maze learning.

These remarks should not be construed as detracting in any way from the value of Anderson's paper. On the contrary, I hope they will serve to clarify the logic of a productive theoretical contribution.

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[MS. received June 19, 1941]

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